SCIENCE

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CONTENTS:

*ZOOLOGY AND THE PHILOSOPHY OF EVOLUTION.**

"I have nothing to say to any Philosophy of Evolution. * * * Attempts to cons ruct such a philosophy may be useful, but in my judgment they are premature."—HUXLEY: 'Collected Essays,' V.

THE facts given in the last two lectures seem to show that we cannot expect much from the 'Lamarckian factors,' even if they should prove to be factors; and while this impression may be wrong, it seems to be the rational frame of mind until it has proved wrong.

He who follows the current literature of zoology finds that many writers assure him, in effect, that the years which Darwin and Wallace gave to hard labor on the problem of species were thrown away, since all they tried to find out by hard work might have been deduced from the Philosophy of Evolution.

We were warned, long ago, that "whoever, unable to doubt and eager to affirm, shall establish principles, and, according to the unmoved truth of these, shall reject or receive others, * * * he shall exchange things for words, reason for insanity, the world for a fable, and shall be incapable of interpreting."

In 'philosophy' current history is sometimes ancient history, and the ardent dis-

* One of a course of lectures on the Foundations of Zoology as delivered in Columbia University, December, 1898.

ciples of 'philosophers' who, in modest earnestness, undertake to formulate the scientific knowledge of their day often become bolder than their teachers, and, growing arrogant and reckless with success, find at last that they have sold their birthright in nature for what proves, when examined, to be no better than a mess of pottage.

The evidence that living matter is continuous, from beginning to end, is so conclusive that it convinces all who know its value. All living things are one by birth, and the system of living nature is, historically, a unit, a consistent whole—not a collection of isolated and independent species. How does it happen, then, that at every point in its history we find it divided into detached groups, separated by gaps and characterized by fitness? Why is the system of living nature such that we cannot picture it as a circle, spreading in all directions from a common center, and growing wider around its whole circumference? Why is it such that it is more exactly represented by a number of growing radii, independent at their outer ends.

This is the problem which Darwin undertook to solve, by showing that it results from extermination according to a standard of fitness. How does the Lamarckian meet it? Sometimes by denying the existence of fitness. Sometimes by asserting, even in the same breath, that fitness is universal and necessary, and that there is no real problem.

He asserts that it is the outcome or expression of a deeper principle of necessary progress or evolution, which must result in fitness. The tendency to regard natural selection as more or less unnecessary and superfluous, which is so characteristic of our day, seems to grow out of reverence for the all-sufficiency of the philosophy of evolution, and pious belief that the history of living things flows out of this philosopy as a necessary truth or axiom.

"The inheritance of characters acquired during the life of the individual is an indispensable axiom of the monistic doctrine of evolution."*

The writer yields to no one in admiration of the doctrine of evolution. So far as it is a scientific generalization from our knowledge of nature, it is one of the greatest triumphs of the human mind, rivalled only by its reciprocal, the doctrine of dissolution.

Experience seems to show, very clearly, that our system of nature is, on the whole, moving towards what commends itself to our minds as evolution, or progress to greater and greater perfection. While there is just as much evidence that each step in evolution is also a step toward dissolution, we have the same rational ground for expecting that this movement will continue, without any sudden radical change, that we have for other expectations which we base on knowledge of nature.

So far as the doctrine of evolution is based on knowledge, it is not only a part, but one of the most valuable and suggestive parts of the system of science, for the scientific law of evolution is part of science; but the philosophy of evolution is held by many as a creed, superior to and able to direct science. As men of science, we, like Huxley, have 'nothing to say to any philosophy of evolution,' except so far as it stands in the way of scientific progress.

We are sometimes told that while the other idols of which Bacon warned us are still worshipped, the idols of the theater have been deserted, and their temples abandoned; although he himself lays peculiar stress on their persistency.

"Lastly, there are idols which have crept into men's minds from the various dogmas of particular systems of philosophy, * * * and these we denominate idols of the theater. For we regard all the systems of philosophy hitherto received or imagined as so many plays brought out and performed, creating

* Haeckel, 'Monism,' p. 96.

fictitious and theatrical worlds. Nor do we speak only of the present systems, or of the the philosophy and sects of the ancients, *since numerous other plays of a similar nature can still be composed.*"

They who worship this modern idol of the theater hold that everything which has taken place and everything which can take place in our universe is deducible from the primal distribution of matter and energy. They tell us that everything in the past and everything in the future follows, of necessity, from this starting-point, inasmuch as it might all have been predicted; but while science knows laws—laws of evolution and others—it knows no necessity except the logical necessity for stopping when evidence stops.

The evolutionist tells us that if we start with a homogeneous universe, with all the matter uniformly distributed, and all the energy kinetic; and if any break in this indefinite unstable homogeneity exist or be brought about, all the rest must follow of necessity, as a matter of course, from the nature of things; that all things must go on along their predetermined course until all the matter shall have fallen into stable equilibrium, and all the energy shall have become latent or potential.

As no one can say the basis for all this is not true, and as it seems much more consistent with scientific knowledge than other systems of philosophy, we must admit that, for all we know to the contrary, it may be true; and we must ask whether, if true, it is any substitute for science; although we must remember that there is no end to the things which, while no one treats them seriously, may nevertheless be true.

All the fancies of the poets which do not involve a contradiction may be true; but while anything which is not absurd may be good poetry, science is founded on the rock of evidence.

Many have found the opinion that all nature is conscious and endowed with volition, that the morning stars sing together, that the waters laugh, that trees talk, and that the wind bloweth where it listeth, worthy of belief; and it is clear that we cannot oppose any belief of this sort by evidence, or convert the sailor who believes that the wind obeys his whistle, by asking for proof.

The path of scientific progress is strewn with beliefs which have been abandoned for lack of evidence, as burst shells strew a battlefield, and it is our boast that they are abandoned, and not lugged along the line of march. As a shell which has failed to burst is, now and then, picked up on some old battlefield, by some one on whom experience is thrown away, and is exploded by him in the bosom of his approving family, with disastrous results, so one of these abandoned beliefs may be dug up by the head of some intellectual family, to the confusion of those who follow him as their leader.

So far as the philosophy of evolution involves belief that nature is determinate, or due to a *necessary law* of *universal* progress or evolution, it seems to me to be utterly unsupported by evidence, and totally unscientific.

This system of philosophy teaches that, for purposes of illustration, our universe may be compared to an unstable, homogeneous, saturated solution, which remains unchanged so long as it is undisturbed, but crystallizes when shaken. The process of evolution must be supposed to start with a disturbance or shock. Something, inherent in the nature of things or outside, must press the button; but matter and its properties do all the rest, just as crystallization follows from the properties of the solution. Even if all this is granted, it is not apparent that the mind of the evolutionist has any power by the aid of which it could deduce anything whatever from

homogeneity, even if it were present at the beginning.

There are homogeneous solutions of sugar and homogeneous solutions of brine, and no one without experience of similar facts has any way to tell what potencies are latent in a solution except by finding out. While we find no reason to suppose a homogeneous saturated solution has any power to initiate anything, we cannot think of it as inert. It is, as it were, alive with energy, and its inactivity is due to the exact balancing of all its powers. It is prepared to spring into energetic action the instant the bonds that chain it are broken by something that disturbs the balance and sets its forces free.

So, too, the primæval homogeneity of the evolutionist is imagined as instinct with world-producing energy, ready to evolve stars and systems and worlds and oceans and continents and living things and men, and all that is 'in the round ocean, and the living air, and the blue sky, and in the mind of man,' the instant it is set free; and so on to the end, which will come when all the energy has worked itself out in motion, and all the matter has found rest in stable equilibrium.

Unless he who worships this idol of the theater is prepared to assert that there is only one kind of indefinite incoherent homogeneity, and unless he knows, in some way of which men of science are ignorant, what sort of homogeneous solution our universe was at the beginning, the only way for him to learn what potencies are latent in it is to find out by studying their products. It is hard to see how he can deduce anything whatever from his necessary law of universal progress except what he discovers. If his premises are admitted, all he can deduce from them regarding our subject is that, if he finds natural selection, the potency of natural selection was latent in his solution.

The philosophy of evolution is of no more use as a substitute for science than any other system of philosophy, although it is, no doubt, not only the latest, but the most consistent with our knowledge of nature, and although it may, for all I know to the contrary, be true. All this fails to give it any value as a short cut to natural knowledge.

The true believer may say, however, that while our finite, imperfect minds may be unable to deduce anything from homogeneity, in the absence of knowledge drawn from experience, the outcome of the process must nevertheless be determinate. As it has all come out of the primæval homogeneity, he says this must have contained it all potentially.

I am no philosopher, but this does not seem obvious or necessary to me. Nature, as we know it, consists in the main of permutations and combinations. 'I do *not* know,' is one thing, and 'I *do* know not,' is another, even if some fail to discriminate.

"It is easy to perceive that the prodigious variety which appears, both in the works of nature and in the acts of men, and which constitutes the greatest part of the beauty of the universe, is owing to the multitude of different ways in which its several parts are mixed with or placed near each other."

When we say three dice *can* be thrown in only two hundred and sixteen ways, all we mean is that we *cannot* throw them in any other way. We cannot throw three zeroes, or three sevens, in any way with ordinary dice without changing the marks; but we cannot attribute to the dice any latent capacity for being thrown in any way, or any capacity to do anything whatever as dice, even after we have been informed by Haeckel that 'the real maker of the organic world is, in all probability, a tetrahedron.' *

* 'Monism,' pp. 27, 28.

Except for a few odd thousands of quintillions of permutations and combinations no others can be formed from twenty-six letters, and if Galileo means any more than this by his remark that all truth is contained in the compass of the alphabet; if his words are more than figurative; if he intends to assert that the potency of literature is latent in the alphabet, independently of an author—it seems to me, with all respect for Galileo, that he is talking nonsense; for while the production of a learned treatise by the fortuitous concourse of letters may not be impossible, all the books we know of have come about in another way.

Twenty-eight figures are required to express the number of distinct deals in whist. "If the whole population of the world, say one thousand millions of persons, were to deal cards day and night for a hundred million years," they might justify Sarah Battle's criticism of the game, but they would not in that time have exhausted one hundredth thousandth part of the possible deals.

It is not clear to me that combinations are latent in the things combined. In fact, the bearing of these things on the matter seems to be negative and passive, rather than active or positive.

It is not clear that, with all their latent potency, a pack of cards would ever evolve a single hand without a dealer; but if a part of the universe, so trivial and insignificant, presents opportunities so boundless, the matter and motion of our universe may present to a dealer opportunities for universes without end, no one like another, I do not see how one can assert that anything in the material universe is necessary or predetermined, except so far as it is one among an infinite number of possibilities.

Huxley tells us that, "if the fundamental proposition of evolution, that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed," be true, "it is no less certain that the existing world lay, potentially, in the cosmic vapor; and that a sufficient intelligence could, from a knowledge of the properties of the molecules of that vapor, have predicted, say, the state of the fauna of Great Britain in 1868, with as much certainty as one can say what will happen to the vapor of the breath in a cold winter's day."

The thoughtful reader will note that Huxley's assertion that, if this proposition be true, it is no less certain that the existing world lay, potentially, in the cosmic vapor is no admission that the proposition is true, or the deduction certain; nor must we forget that the most notable and valuable characteristic of Huxley's teachings is the declaration, in all his works, of the truth that the scientific basis of our confidence in the order of nature is evidence.

Again and again, in words which are unmistakable, he tells us that, while we may have reasonable confidence what to expect from the vapor of our breath in a cold winter's day, we know nothing about it except what has happened. The scientific value of our confidence depends, he tells us, on the extent of our experience of the behavior of the vapor of our breath, and similar bodies, on a cold day, or under similar circumstances. As, in this case, our experience is pretty extensive, the deduction is safe and reasonable; but when a young man who had passed his life in the tropics spent the night on top of a high mountain with my students he was so far from deducing anything from the frosty morning air that he was at first alarmed by the behavior of the vapor of his breath.

If Huxley is right; if the logical basis for confidence in nature is evidence, it seems clear that no amount of knowledge can ever give it any other basis; for nothing seems more obvious, or more strictly logical, than

our inability to deduce anything from a single experience. The burnt child may dread the fire as much as if it had been burned twenty times, but the only way for it to learn whether, and to what degree, its dread is wise and prudent, without passing through the slow and painful process of selection, is to get knowledge, for a single experience affords no basis for any logical process.

While the emotional value of a sensation is, no doubt, limited by inherited structure, and dependent, to some degree, on intensity, its objective value as knowledge is regulated in accordance with the statistical law of probability.

If the history of what we call *our* universe were complete from beginning to end; if everything which exists in it were reduced to mechanical principles, and traced back to primitive nebulosity, this history would be only a single experience in cosmogeny, so far as the history of universes is in question. If we were to find, somewhere, a second nebulosity we would not be able to infer anything, except from the worthless analogy of a single experience; nor would we be able to infer or deduce, from our own, anything, not already known, with more than reasonable confidence. If we were still ignorant of any part of our order of nature we should have no way to find out but the way we have now, and while our confidence in its stability would be reasonable and judicious it would not be necessary or absolute unless our experimental knowledge were also absolute.

It seems to me that the truth for which Huxley strives, and hits with imperfect aim, would be more correctly expressed by the statement that, if our knowledge of nature were to be made complete, from beginning to end, we should expect to find that our confidence in its stability had been reasonable and judicious, and wise throughout, and that any other expectation would have been folly and suicide, bodily as well as mental, and that it is only in this sense that we could assert that it all lay potentially in the cosmic vapor.

It is not because I dread or fear the philosophy of evolution that I refuse to accept it, but because it is not yet proved. When it is proved I shall accept it with cheerfulness, for I most assuredly hold no belief which is inconsistent with it, although I fail to see how the reduction of all nature to mechanical principles could show that nature is determinate; for if exhaustive knowledge of 'primitive nebulosity' should sometime show that there is nothing in nature which might not have been expected, I cannot see how this could show why the things we expect should be the things which come about.

They who assert that complete knowledge would be fore-knowledge forget that, for minds like ours, the only source of knowledge, either complete or incomplete, is evidence; for evidence can tell us only what has happened, and it can never assure us that the future *must be* like the past. Even if we knew all that has happened, from the beginning down to the present moment, we should have to regard the unknown remainder as equal, in all probability, to the known past. To my mind, Jevons' demonstration that if certainty be represented by unity the utmost confidence we can ever reach by complete knowledge can never exceed a value of one-half seems conclusive; but even if it be increased until it differ from certainty by less than any assignable quantity it must still remain nothing but reasonable confidence.

There may be some unknown reason why the stone which I set free from my hand shall fall, and it may be that, as my mind has been shaped by natural selection, I am unable to expect anything else than that it shall fall; but science affords no evidence that its

fall is necessary or predetermined ; for most thoughtful students assure us that the inductive study of nature tells us nothing about it, except that, so far as we know, all stones so placed have fallen according to Newton's laws, and that we have not the smallest reason to expect that any stone so placed will act differently; nor, so far as I can see, would prove that all nature is mechanical, from beginning to end, be inconsistent with belief that everything in nature is immediately sustained by Providence; nor am I able to see how it would be inconsistent with my conviction that my volition counts for something as a condition of the course of events.

I have tried to show that, while the responsive activities of living things do not take place unless they are called forth by a stimulus, the things which they do under a stimulus are no more than their organic mechanism would lead one to expect, and that there is no necessary antagonism between those who attribute the development of the germ to mechanical conditions and those who attribute it to the inherent potency of the germ itself.

I have also tried to show that there need be no more antagonism between those who attribute knowledge to experience and those who attribute it to our innate reason ; for, while knowledge does not arise in our minds without a sensible occasion, the knowledge which does thus arise may be no more than one who knew the whole natural history of our minds might have expected.

We must now ask whether proof that all nature was latent in the cosmic vapor would be inconsistent with the belief that everything in nature is immediately intended rather than predetermined.

Certain monists tell us that the scientific doctrine of evolution is the same as Pantheism, for "since the simpler occurrences of inorganic nature and the more complicated phenomena of organic life are alike reducible to the same natural forces, and since, furthermore, these in their turn have their common foundation in a simple primal principle pervading infinite space, we can regard this last [the cosmic ether] as all-comprehending divinity, and upon this found the thesis: Belief in God is reconcilable with science."*

They who agree with Haeckel may worship stones, if they see fit; but they seem to me to fail as completely as any South Sea Islander to understand the nature of scientific evidence ; for it is one thing to find sermons in stones, and quite another to see a divinity in the stone itself, 'which, if with reason, we may do, then let our hammers rise up and boast they have built our houses, and our pens receive the honor of our writings.' But everything must be determinate, says the pious evolutionist, or what would become of the fixed order of nature? Among the things that occupy the biologist are such aspects of nature as life, and consciousness, and volition, and reason, and right and wrong. Whatever these things mean, they are part of nature, and the zoologist cannot push them out of sight if others may. He does not know what their places in the system of nature are, but he would like to find out; and he knows no way to find out except to discover.

When they who worship at the shrine of evolution tell him there can be no spontaneity in nature, because the order of nature is fixed and unchangeable, he asks what reason there is for thinking that proof that everything in nature is mechanical, and no more than might have been expected, would show that anything is fixed, or predetermined, or necessary.

Science has nothing to do with the notion of 'necessity,' and is quite content to leave it in the hands of its originators, the metaphysicians and theologians and 'philosophers,' who alone are responsible for all the mental confusion it has brought about.

* Haeckel, 'Monism.'

What the man of science asserts is that he will not admit that anything is 'arbitrary.' "It was the ignorance of man's reason that begat this very name, and by a careless term miscalled the Providence of God; for there is no liberty for causes to operate in a loose and straggling way."*

Belief that everything in nature is mechanical is neither more nor less than belief that everything in nature is orderly and what might have been expected; and if any one thinks that discovery that things do take place in order is any reason why they should, his distrust of science is only reasonable; for science is not for such minds as his.

It is in my mind to ask a question. Will any amount of knowledge of matter and motion tell the evolutionist whether I shall ask it or pass it by and go on to another subject? If he answer Yes I ask my question: How does he know? If he assure me that a being so reasonable as I am known to be will not ask anything that might not have been expected I thank him for the compliment, for I try to be a reasonable creature. But if he assert that his confidence in my thoughts and actions proves that they are *necessary* I must ask him how he knows; for I fail to see how proof that an event is mechanical and neither less nor more than might have been expected shows that it is *necessary;* nor can I see any more reason why my confidence in my freedom proves that my acts are *arbitrary.*

The man of science quarrels with no man's opinions, but he will not be held responsible for perplexities which are none of his making.

I am unable to share the dread of the evolutionist that the basis of science may be destroyed if we do not admit that all nature must be determinate. All agree that the past is determinate, so far as the word means anything to us, and there seems to be valid ground for the belief that every part of the material universe contains a permanent record of every change which has ever occurred in any part.

"If on a cold polished metal, as a new razor, any object, such as a wafer, be laid, and the metal be breathed upon, and, when the moisture has had time to disappear, the wafer be thrown off, though now the most critical inspection of the polished surface can discern no trace of any form, if we breathe once more upon it, a spectral image of the wafer comes plainly into view, and this may be done again and again. Nay, more, if the polished metal be carefully put aside, where nothing can deteriorate its surface, and be kept so for many months, on breathing upon it again the shadowy form emerges. A shadow never falls upon a wall without leaving thereupon a permanent trace, a trace which might be made visible by resorting to proper processes. Upon the walls of our most private apartments, where we think the eye of intrusion is altogether shut out, and our retirement can never be profaned, there exist the vestiges of all our acts."*

Babbage has pointed out ('Ninth Bridgewater Treatise,' pp. 113–115) "that if we had power to follow and detect the minutest effects of any disturbance each particle of existing matter would furnish a register of all that has happened. The track of every canoe, of every vessel that has as yet disturbed the surface of the ocean, whether impelled by manual force or elemental power, remains forever registered in the future movement of all succeeding particles which may occupy its place. The furrow which it left is, indeed, instantly filled up by the closing waters, but they draw after them other and larger portions of the surrounding element, and these again, once moved, communicate motion to others in endless succession. The

* Religio Medici.

* Draper, 'Conflict of Science and Religion.'

air itself is one vast library, in whose pages are forever written all that man has said or even whispered. There, in their mutable but unerring characters, mixed with the earliest as well as the latest sighs of mortality, stand forever recorded vows unredeemed, promises unfulfilled, perpetuating in the united movements of each particle the testimony of man's changeful will."*

So far as we know, nothing that has ever been can be as if it had not been; and we seem to have good ground for believing that every portion of the material universe contains a record of every change that has taken place in all its parts, and also for believing that there is no limit to the power of minds like ours to read and interpret this record. Every new experience also shows that our expectation that the future will, on the whole, be like the past is reasonable. In these facts science finds a basis broad enough and firm enough for all our needs; for to this extent the data of science are latent in the physical universe, even if the future is, in part, to be what man and other living things make it.

If these evolutionists who hold that all nature is determinate and necessary are right, mind would seem to be useless. It may, for all I know to the contrary, be true that when I perform an action because my reason approves it neither the performance of the action nor the approval of my reason is anything more than exhaustive knowledge of the mechanism of my brain might have led one to expect; and if it follows that my action is necessary, and must take place, whether my reason approve it or not, reason would seem to be useless; but I cannot see why this should follow, for I fail to see how or why proof that my reason is mechanical and no more than might have been expected from my structure should be inconsistent with my confidence in its value, since I cannot conceive how this proof could show that it is necessary, or predetermined, or useless.

*Quoted by Jevons, 'Principles of Science,' p. 758.

I know the value of my reason by what seems to me the best of all evidence. If it were proved useless I should be quite ready to believe; but the improbability of this opinion seems to me so much like impossibility that I must ask for proof which is correspondingly conclusive; for I most assuredly refuse to give any weight to the 'faith' of pious evolutionists, and I must insist on my right to demand more evidence if more is to be had, for I cannot accept the mind of the evolutionist as a measure of nature.

Living things are continually bringing about rearrangements of matter and motion which would never, so far as I can see, have come about without them, and many of the things which they thus bring about are useful to the beings which bring them about. The earth would be very different in many respects if man had never inhabited it, and the effects of his activity will last as long as matter, whatever may be his fate. His influence upon the earth would have been very different if the plants of Carboniferous times had not stored up solar energy and worked their changes in matter millions of years ago. If the dodo, and the great auk, and the halicore, and the American bison, could tell their story they would bear witness that man is a factor in the order of nature.

They who are discontented with reasonable or 'moral' certainty, and tell us they want absolute certainty, must find this sort of certainty if they can and where they can, but their words seem strange to the zoologist. He knows that the rocks are full of the remains of organisms which passed out of existence because they were born in evil times, when the adjustments to the order of nature, which had served the purposes of their ancestors for millions of years, ceased to hold good.

If our race should ever find itself where the old order changes; if our reasonable expectations should disappoint us; if what we call the 'order' of nature should prove to be no more than natural selection would lead us to expect, and if a different selective standard should sometime modify this order, every zoologist knows that the human species would not be the first to meet this evil fate.

If, with Aristotle, we believe 'that is natural which holds good;' if, with Erigena, we hold that nature is the sum of all things, we cannot believe that life and consciousness and reason and volition are anything but part of nature. The question the zoologist would like to answer is what their place in nature is. So far as I am aware, no one believes that these aspects of nature exist in themselves, without antecedents, for we know that many of their antecedents are physical, and we want to find out, if we can, whether this is true of all of them or not. For my own part, I fail to see what bearing this wish has on the question whether the order of nature is 'fixed' or unfixed; nor can I see how proof that the conditions which, being given, are good reasons for expecting reason or the moral sense are mechanical should show that reason and morality are useless.

They who take refuge in an imponderable ether as soon as they find it difficult to discover, in ponderable matter, the key to all the antecedents to certain phenomena of light and electricity have no reason to cry out that the fixed order of nature is threatened, because the modest zoologist has not yet been able to find, in ponderable matter and physical energy, the key to all his problems.

Berkeley tells us that human knowledge has its basis in experience, and that its scientific value is to be measured by the amount of this experience; and Huxley assures us that there is but one kind of knowledge and but one way to acquire it. They hold our practical test of truth to be evidence, although a pious evolutionist, who admits that, for all he knows, they may be right, is a heretic; for Herbert Spencer tells him that the Philosophy of Evolution stands or falls with the assertion that the ultimate criterion of truth is inability to conceive its negative.

If you will read Part VII. of his 'Principles of Psychology' with care you will note that its author tells us that unless we admit this we cannot be his disciples. It is not enough to admit ignorance of things ultimate, or to confess that, for all one knows, inability to conceive its negative may sometime prove to be the ultimate criterion of truth. One may admit that he is unable to discover any line which separates the responsive actions of living things in general from the rational actions of thinking men; that he does not know how or where instinct and impulse and emotion give place to reason. One may have as little faith in the idealism of Berkeley as he has in Spencer's realism, or in the materialism of German physics, or in the monism of the psychologists; but unless he knows what the relation between mind and matter is he cannot join the throng of worshippers before the shrine of this modern idol of the theater, for its leader tells him that suspension of judgment on this difficult question is as fatal as disbelief.

Proof that we should not be here if our remote ancestors had not responded to the order of nature as they did is no proof that our minds are a measure of nature, or that our responses will be valuable in the future, or that nature is determinate.

Now the difference between belief that the ultimate test of truth is the inconceivability of its negative, and belief that our practical test of truth is evidence, is this: that while inability to conceive the negative of a proposition may be absolute to us, as

nature has made us, at our present intellectual level, evidence is progressive, and can afford no basis for ultimate philosophy.

Our pre-Cambrian ancestors may have been unable to conceive the negative of many propositions, but what does the inability of a turnip or a sponge to conceive the negative of Newton's laws signify? Or what would our own inability signify if we should sometime find out that the ponderable matter which makes up what we call 'our universe' has been sifted out or segregated from other forms of matter by its property of weight? For no less distinguished an authority than Herschel held that there is proof of the existence of levitative matter as well as gravitative matter.

One volume of Herbert Spencer's 'Philosophy' is devoted to proof that we primarily know objects, but to this long argument Berkeley answers: Granted. Most assuredly we primarily know objects, but he tells us that the objects we know primarily are objects of sense.

So the frozen river of philosophy grinds on, scratching the surface of the everlasting hills, and melting before the genial sunshine of science, only to receive new accretions from the unknown and frozen space beyond the snow-line.

Some fifteen hundred years have passed since we were told by Proclus that "there are two sorts of philosophers. The one placed body first in the order of beings, and made the faculty of thinking depend thereupon, supposing that the principles of all things are corporeal; that Body must really or principally exist, and all other things in a secondary sense, and by virtue of that. Others making all corporeal things to be dependent upon Soul or Mind, think this to exist in the first place and primary sense, and the being of Bodies to be entirely derived from and to presuppose that of Mind.*

* Berkeley, 'Siris,' p. 263.

While the modern psychologist tells us that there is a third point of view, and that, for all we know to the contrary, both mind and matter may ultimately prove to be phenomenal; that all mind may be matter in motion, and all matter in motion mind, or at least the raw material of mind, I cannot see why the admission of this possibility compels us to take a side and make a choice; for may we not find a fourth alternative, in a humble confession that, while we do not know what the relation between mind and matter is, we wish to find out? "And, although it may, perhaps, seem an uneasy reflection to some that, when they have taken a circuit through so many refined and unvulgar notions, they should at last come to think like other men; yet, methinks, this return to the simple dictates of nature, after having wandered through the wild mazes of philosophy, is not unpleasant. It is like coming home from a long voyage: a man reflects with pleasure on the many difficulties and perplexities he has passed through, sets his heart at ease, and enjoys himself with more satisfaction for the future." *

If the antecedents to consciousness are outside consciousness it seems no more than natural that we should be unconscious of them; and the zoologist who admits that he does not know whether they are or are not all to be found in that part of the universe which may be made manifest to sense does not feel guilty of a threat to the fixed order of nature, or to anything or anybody else.

There are two reasons why biology and the 'Philosophy of Evolution' should be associated.

In the first place, there is a wonderful analogy between the problems of the sensible universe and the unfolding of the latency of the germ into the potency of the fully developed living being. It is not impossible that the key to the more specific

* Berkeley, Preface to 'The Three Dialogues.'

problem may fit the lock which seals the greater.

In the second place, the two subjects are historically associated. So long as men believed that species are distinct creations, no philosophy of evolution could have gained general acceptance. By convincing all thoughtful persons that species have a history which may be studied by scientific methods, Darwin led many who would not otherwise have given it a hearing, to treat the new philosophy with respect, but natural science is not 'philosophy,' notwithstanding this intimate historical connection between the proof that species are mutable and the spread of belief in the 'Philosophy of Evolution.' I have selected the passage which I have put at the head of this chapter in order to show that the view of the matter which is here set forth is not new, even among advanced biologists.

Huxley's attitude will, no doubt, be a surprise to many who think they have read his books with diligence. He continually calls himself an 'Evolutionist,' and he can hardly blame a reader who, failing to draw nice distinctions, holds him to be one of the chief pillars in the temple of the new philosophy. Some confusion may be permitted to those who remember his public lectures on 'Evolution,' his essays with the same title, and his declaration that the work of his life has involved him 'in an endless series of battles and skirmishes over evolution.'

It is easy for one who understands his true position to see that his essays lend no countenance to the opinion that he has ever been or sought to be either a pillar or a disciple of any system of philosophy, for he has never ceased from affirming his ignorance of many of the subjects which philosophy seeks to handle.

His evolution is not a system of philosophy, but part of the system of science. It deals with history—with the phenomenal world—and not with the question what may or may not lie behind it.

During the last half century natural science has become historical. We have opened and learned to read a new chapter in the records of the past. The attributes of living things, which seemed to the older naturalists to be complete and independent in themselves, have proved to have a history which can be studied by the methods of science. They have been found to be steps in a long sequence of events as orderly and discoverable as the events which are studied by the astronomer or the geologist.

The cultivation of natural science in this historical field, and the discovery that the present order of living things, including conscious, thinking, ethical man, has followed after an older and simpler state of nature, is not 'philosophy,' but science. It involves no more belief in the teachings of any system of philosophy than does the knowledge that we are the children of our parents and the parents of our children; but it is what Huxley means by 'evolution.'*

His lectures on 'Evolution' deal with paleontology, and narrate facts which are found in every text-book on the subject; but natural science, as it is taught in the text-books on botany and zoology and embryology and paleontology, is, most assuredly, no 'Philosophy of Evolution.' It fell to Huxley to fight and win a battle for science; and while he himself calls it a battle for evolution, his use of the word need mislead none, although it has misled many.

One word in its time plays many parts, and the word 'evolution' has had many meanings. To-day, in popular estimation, an evolutionist is not a follower of Bonnet; nor one who is occupied with the binomial theorem, or with the evolutions of fleets

*See Huxley, 'Essays,' V. i., pp. 44-54.

and armies. Neither is he a cultivator of natural science. Whatever the word may have meant in the past, it has, in common speech, come to mean a believer in that philosophy of evolution which, according to such evolutionists as Huxley, is 'premature.' Since this is so, and since the growth of language is beyond individual control, would it not be well for them to stand where Huxley stands, and 'have nothing to say to any philosophy of evolution,' to stop calling themselves 'Evolutionists,' and to be content with the good old name of 'Naturalist?'

To the pious evolutionist, who asks what will become of the fixed order of nature if we are not convinced that everything is determinate, we answer that, while this sort of reasoning is not new, it has a strange sound in the mouth of a student of science. The order of nature has outlasted many systems of philosophy, and it may survive others. We have found our astronomy and our geology and our law of the mutability of species, and none of the dreadful things predicted by 'philosophers' have come about. There may still be more things in heaven and earth than are dreamed of in 'philosophy.'

History warns us that, as the price of progress in science, all the idols of the theater, and all other idols, "must be abjured and renounced with firm and solemn resolution, and the understanding must be completely freed and cleared of them, so the access to the kingdom of man, which is founded on the sciences, may resemble that to the kingdom of heaven, where no admission is conceded except to children."

If the world thinks hard names are the just due of them who assert their living wish to know, while humbly confessing ignorance, the biologist must bear up as well as he can if he is called a 'scientific Rip Van Winkle,' or an 'agnostic,' or even 'a turbaned and malignant Turk.'

If we seek admision to the temple of natural knowledge naked and not ashamed, like little children, hard names cannot hurt us, nor need they scare us.

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FERMENTATION WITHOUT LIVING CELLS AND SYNTHETIC PROTEIN.

I TAKE pleasure in complying with the request of the Editor to furnish the readers of SCIENCE with a brief abstract of the papers read at the late Vienna Congress by Professor Buchner, of Tübingen, and Dr. Lillienfeld, of Vienna, on 'Fermentation without Cells' and 'The Synthesis of Albumenoids,' respectively. The paper of Professor Buchner was presented to the whole Congress on the occasion of the first general meeting, July 28, 1898. The paper was illustrated with numerous experiments showing the production of vigorous fermentation within the time occupied by reading the paper, secured by ferments entirely free of yeast cells. The active principle of the yeast cells is obtained by grinding the yeast with quartz sand, for the purpose of disrupting the cells, and subsequently submitting the moist mass to a high pressure, viz: 500 atmospheres. Nearly all the yeast cells are disrupted by this process, and a microscopic examination of the residue discloses the empty cells from which all liquid contents have been removed. A more complete evacuation of the contents of the cells is secured by breaking up and moistening the press cake and repeating the grinding and pressure. About half a liter of liquid is obtained from a kilogram of yeast. The liquid contents of the cells, as they come from the press, are filtered through fine paper, in order to remove any danger of whole yeast cells being found in the extract.

The resultant liquor is clear or slightly opalescent, has a yellowish color and the pleasant odor of yeast. It contains con-

siderable quantities of carbon dioxid and some coagulable albumen. That the prepared juice contains enzymes is easily shown by the hydrogen peroxid test. Hydrocyanic acid has the property of forming a very unstable compound with these enzymes whereby the action of hydrogen peroxyd is prevented, and scarcely any oxygen is given off from such a mixture until the acid has been partly or entirely removed by a current of air. Of the enzymes present invertin has been detected, and it is probable that another ferment capable of hydrolyzing maltose and glycogen is present.

Most interesting is the deportment of the yeast juice towards sugars. Fermentation is set up much quicker than by yeast and proceeds much faster. Quite a number of sugar solutions treated with the ferment at the beginning of the paper were in rapid action before the close. The evolved gas is almost pure carbon dioxid. The reaction is made much quicker if a small amount of sugar is dissolved in large volume of the yeast liquor. The vitality of the ferments continues for two or three days, after which time their activity is rapidly diminished. When carefully dried at a low temperature the vitality of the ferments is not destroyed, and it is probable that in a desiccated state the active properties of the mixture may be kept indefinitely without loss.

It follows, as a result of these investigations, that living cells are not necessary to fermentation, and thus another of the fetiches of the old chemistry is destroyed. Fermentation can no longer be regarded as a physiological act produced by living organisms. It is simply due to the chemical power of an amylolyte acting in a manner entirely similar to the ordinary digestive ferments. It has not yet been possible to isolate the fermentative enzyme, partly because of its instability, but chiefly because of the presence of other enzymes, such as invertin and the oxydases, which deport themselves analytically in the same manner as the fermenting bodies.

In regard to the practical uses which may be made of this discovery little can be said. By reason of the fact that the fermentative organisms can be preserved in a dry state, it ought to be possible to secure a more general distribution of those particular yeasts which give the highly-prized flavors to beers and even to wines. If this be the case the flavors which are produced in the great breweries of the Old World might be produced under proper conditions in the breweries in other countries. It would be wise, however, to postpone any discussion of the practical applications of the discovery of fermentation without living cells until the matter has been more thoroughly worked out from the purely scientific point of view.

The paper of Dr. Lilienfeld, as has already been intimated, refered to the synthesis of a nitrogenous body having the properties of a peptone or even of an albuminoid or proteoid. The synthesis of such a peptone or peptonoid marks a distinct step forward in synthetical work in the field which has already been partly developed by Grimaux, Pickering, Williamson and others.

We can only now speak, however, of its centesimal composition. The state of its molecular condensation and atom position can only be determined by securing large quantities of the product and submitting it to chemical and digestive studies. It is probable that, as in the case of sugars, the artificial peptone will lack the vital element. In other words, while the chemist has succeeded in building molecules which resemble, in every outward respect, those built up by Nature, they are uniformly dead, without cell functions or cell activity. The details of this important scientific work must be awaited before a final judgment in regard to its far-reaching importance can be formulated.

The synthesis of peptone is effected by the condensation of phenol with glycocoll with the help of phosphoroxychlorid. A hydrochlorate of peptone results, which gives all the characteristic reactions of protein. By conversion into sulfate and the decomposition of the latter the free peptone is obtained, which it is claimed by Lilienfeld is similar both in chemical and physiological properties to the natural product.

It is evident from the method of preparation that the product contains no sulfur, since the only sulfur-containing ingredient used was sulfuric acid, and this could not possibly enter into the organic preparation. Granting that a peptonoid body was produced, the synthesis of a true proteid, which must contain sulfur, is still undemonstrated.

The color reactions which are supposed to be characteristic of protein must not be relied on too surely. They are probably due to decomposition, and not to the action of the molecule as a whole. It is stated by Pickering that a mixture of tyrosin, indol and biuret will give all the reactions characteristic of a proteid. If the prospects of artificial food depended on these so-called synthetic products the vocation of the geoponist would be assured for many millions of years to come.

The interesting fact, however, in papers of this kind is found in the accomplishment of steps which a few years ago were considered improbable or impossible. It is certain that the chemist is now able to produce organic compounds, or bodies which closely resemble them, in great numbers, if not in considerable quantities. Practically, such investigations will lead to further studies in the domain of synthetic chemistry and doubtless to the discovery of many additional synthetic products of great utility. In so far as the production of artificial food is concerned, however, there seems to be absolutely no possibility of Nature's methods ever being supplanted or even greatly supplemented by the synthetical products of the laboratory.

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THE 'FEELING OF BEING STARED AT.'

EVERY year I find a certain proportion of students, in my junior classes, who are firmly persuaded that they can 'feel' that they are being stared at from behind, and a smaller proportion who believe that, by persistent gazing at the back of the neck, they have the power of making a person seated in front of them turn round and look them in the face. The phenomena are said to occur in any sort of assembly—at church, in the class room, in a public hall. The 'feeling,' when it is not merely described as 'uncanny,' 'a feeling of Must,' etc., is referred to as a state of unpleasant tension or stiffness at the nape of the neck, sometimes accompanied by tingling, which gathers in volume and intensity until a movement which shall relieve it becomes inevitable. It is believed that this stiffness is, in some way or other, the direct effect of the focussing of vision upon the back of the head and neck.

The belief rests upon a foundation of fact, but (like most popular beliefs) implies a misinterpretation of fact. The psychology of the matter is as follows: (1) We are all of us more or less 'nervous' about our backs. If you observe a seated audience, before it has become absorbed in the music or lecture for which it came together, you will notice that a great many women are continually placing their hands to their heads, smoothing and patting their hair, and every now and again glancing at their shoulders or over their shoulders to their backs; while many of the men will frequently glance at or over their shoulders,

and make patting and brushing movements with their hand upon lapel and coat-collar. This sort of anxiety about the back varies considerably from individual to individual, but most of us are probably aware that we share it to some extent. A friend of mine, who learned to dance after he had arrived at man's estate, told me that it was positively painful to him to turn his back upon the instructor (even during a private lesson), and that it was as positive a relief when he was allowed to face the instructor's back, and posture unseen. Some lecturers are very averse, again, to turning their backs upon an audience, even for the few moments that are required for blackboard writing. It is not difficult to imagine a phylogenetic reason for this shyness, and for the exploring movements of eyes and hands, when we remember that the organs of sight are placed for *forward* vision, and think of the constant care that must have been devoted to the defenseless back when our ancestors first assumed the upright position. But, however that may be, there can be no doubt of the facts at the present day.

(2) Since it is the presence of an audience, of people seated behind one, that touches off the movements described above, it is natural that these movements should in many cases be extended so as to involve an actual turning of the head and sweeping of the eyes over the back of the room or hall. Not only is one nervous about one's appearance as viewed from behind; one is also anxious that this nervousness shall not be apparent. It is not good breeding to be concerned about one's looks in a public place. Hence there is often a voluntary continuation of the original ideomotor movements; one looks round enquiringly, as if one were seeking for a special person or event—taking one's direction from some chance noise of falling seats or rustle of dresses, letting one's eyes come to rest upon some patch of intense color, etc., etc. The details differ in different cases; the general mechanism is the same. Observe that all this is entirely independent of any gaze or stare coming from behind.

(3) Now, movement in an unmoved field—whether the field be that of sight, or hearing, or touch, or any other—is one of the strongest known stimuli to the passive attention. We cannot help but attend to movement; and phylogenetic reasons are again not far to seek. Hence if I, *A*, am seated in the back part of a room, and *B* moves head or hand within my field of regard, my eyes are fatally and irresistibly attracted to *B*. Let *B* continue the movement by looking round, and, of course, I am staring at him. There are, in all probability, several people staring at him, in the same way and for the same reason, at various parts of the room; and it is an accident whether he catch my eye or another's. Someone's eye he almost certainly will catch. Moreover, as there are many others, besides *B*, who are afflicted with *B*'s restlessness, it is an accident, again, whether my neighbors, to right and left, are also looking at *B*, or are looking at some part of the room quite remote from *B*. Both of these accidents, until they are recognized as accidents, evidently play into the hands of a theory of personal attraction and telepathic influence.

(4) Everything is now explained, except the feeling that *B* experiences at the back of his neck. This feeling is made up, upon its sensation side, simply of strain and pressure sensations which, in part, are normally present in the region (sensations from skin, muscle, tendon and joint), but are now brought into unusual prominence by the direction of attention upon them, and, in part, are aroused by the attitude of attention itself. 'Nervousness' about one's back means, psychologically, constant attention to the sensations coming from, and the

mental images of, that portion of the body; and attention, in its turn, means in most cases movement of the part of the body attended to. If one thinks hard of one's knee, or foot, *e. g.*, one will obtain a surprisingly intensive and insistent mass of cutaneous and organic sensations of which one was previously unconscious, or at best but very dimly conscious; while, at the same time, there is an actual twitching or bracing of the knee or foot, which sets up new sensations. Any part of the body will thus yield up its quantum of unpleasant sensation, if only for some reason the attention can be continuously held upon it, to the exclusion of other topics. The 'feeling of Must' in the present case is no more mysterious than is the 'feeling of Must' that prompts us to shift our position in a chair, when the distribution of pressures has become uncomfortable, or to turn our better ear to the sound that we wish particularly to observe.

(5) In conclusion, I may state that I have tested this interpretation of the 'feeling of being stared at,' at various times, in series of laboratory experiments conducted with persons who declared themselves either peculiarly susceptible to the stare or peculiarly capable of 'making people turn round.' As regards such capacity and susceptibility, the experiments have invariably given a negative result; in other words, the interpretation offered has been confirmed. If the scientific reader object that this result might have been foreseen, and that the experiments were, therefore, a waste of time, I can only reply that they seem to me to have their justification in the breaking-down of a superstition which has deep and widespread roots in the popular consciousness. No scientifically-minded psychologist believes in telepathy. At the same time, the disproof of it in a given case may start a student upon the straight scientific path, and the time spent may thus be repaid to science a hundredfold. The brilliant work of Lehmann and Hansen upon the telepathic 'problem' (*Philos. Studien*, 1895, XI., 471) has probably done more for scientific psychology than could have been accomplished by any aloofness, however authoritative.

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WHAT IS SCIURUS VARIEGATUS ERXLEBEN?

WHILE working out the synonomy of the Mexican squirrels I have had occasion to consult the much quoted Historiæ Animalium Novæ Hispaniæ of Fernandez, edition of 1651. The descriptions of birds and mammals in this work have served as the basis for many species named by succeeding authors whose vagueness of description and lack of definite information concerning the geography and animal life of Mexico have resulted in great confusion. At the time when Fernandez made his observations the main area of Spanish occupation in Mexico was the southern end of the Mexican tableland, about the valley of Mexico, and thence eastward across the plains of Puebla, through the Cordillera (crowned by the peaks of Orizaba and Cofre of Perote) to the hot lowlands of Vera Cruz. For several seasons zoological explorations have been conducted in this area by the writer, who, as a result, has become familiar with the topography and resident species of birds and mammals. In the light of this knowledge it is possible to identify, with certainty, many of Fernandez's species, for example his *Quauhtecallotlquapachtli* or *Coztiocotequallin*.*

In 1777 Erxleben, in his Systema Regni Animalis, Mammalia, p. 421, named this animal *Sciurus variegatus*. Since Erxleben derived his information from Fernandez it becomes necessary to learn what the latter says. Following is the translation of Fer-

* Hist. Animalium, p. 8.

nandez's description: "The second is called *Quauhtecallotlquapachtli* or *Coztiocotequallin* from the yellow color of the belly; it grows nearly twice the size;* in color is white, black and brown mixed, except the belly, which is pale or fulvous; it has a very long and hairy tail, with which it sometimes covers itself. It lives in holes in the ground and in enclosed hollows, in which it also rears its young. It feeds on Indian corn, which, taken from the fields, it stores up for winter. It is agile like the others, never becomes tame or lays aside its natural wildness."

The vague ideas prevailing among writers regarding the animal described by Fernandez and Erxleben is evident when it is known that at least twelve well-marked species and sub-species of American squirrels have been referred to it. These squirrels represent species having distinct ranges, lying between the Carolinas in the United States and Honduras in Central America. Since the species was named by Erxleben it has been uniformly treated by authors as a true *Sciurus*. Now let us see what foundation there is for treating this species as a true squirrel. Erxleben places it under his *Sciurus*, but, as he covers in this genus several genera now considered distinct, this furnishes no guarantee of its actual generic position. It is true that he quotes as a synonym the *Coquallin* of Buffon, but this merely shows that, in naming the animal of Fernandez, Erxleben had no very definite idea of it. Erxleben's description, evidently quoted from Fernandez, is as follows: "Magnitudine dupla S. vulgaris. Auriculæ imberbes. Corpus supra nigro, albo et fusco variegatum, ventre flavescente. Cauda supra corpus reflexa." This description might easily refer to a *Sciurus*, but when the author adds the following notes, viz., "Habitat in Mexico. Subterraneus parit, cibumque colligit pro hieme. Edit Zeam. Non mansuescit." it is evident that he is describing a *Spermophilus*.

I think it may be positively stated that no Mexican *Sciurus* has the habits of the animal described by Fernandez. The *Spermophilus macrourus* of Bennett and later authors is an abundant resident throughout the part of the tableland familiar to Fernandez. It is conspicuous about farms, and agrees in habits and colors with the animal described by Fernandez and quoted by Erxleben, and again described by Lichtenstein as *Sciurus buccatus* (Abh. k. Akad. Wiss., Berlin, pp. 115, 117 (1827), 1830). This being the case, it is difficult to see how there can be any reasonable doubt that the *Quauhtecallotlquapachtli* of Fernandez, the *Sciurus variegatus* of Erxleben, *Sciurus buccatus* of Lichtenstein and *Spermophilus macrourus* of Bennett are one and the same animal. Consequently the large, bushy-tailed *Spermophilus* of the Mexican tableland becomes *Spermophilus variegatus* (Erxleben) and stands as the type of the group to which belong *S. couchi* and *S. grammurus*, which are probably races of this species. It was probably about the border of the Valley of Mexico, near the City of Mexico, that Fernandez became familiar with this animal, and we may, therefore, consider this as the type locality.

Note: *Spermophilus mexicanus* (Lichtenstein) is the only other common and widely spread species of *Spermophilus* on the southern end of the Mexican tableland and it is readily recognizable as the *Techallotl* of Fernandez.

E. W. Nelson.

* The context shows that this must refer to the author's Techallotl, which is *Spermophilus mexicanus* (Licht.).

NOTES ON PHYSICS.

TRANSFORMER DESIGN.

A paper by F. W. Carter read before the November meeting of the American Institute of Electrical Engineers gives, for the

first time, a method for the rational design of alternating current transformers. Our knowledge of this important piece of apparatus has, up to this time, consisted in part of a precise and complete knowledge of its behavior and in part of a keen sense of propriety in design on the part of the more progressive practical electricians; but a rational method for correlating the various items in the design with a view to the production of a transformer which shall at once meet prescribed conditions in the best possible way we have not had. The method has rather been to assume (on paper) a large number of alternative designs, to calculate the action of each in detail, and to adopt that design which best meets all the requirements.

It is only fair to the practical electricians to say that Mr. Carter's results will not invalidate much, if any, of their more recent work for the reason that the old method of designing is fully adequate if enough labor is devoted to it, and this condition has been abundantly satisfied. Teachers of electrical engineering, on the other hand, may hail Mr. Carter's paper with satisfaction as affording further occasion for the application of elegant mathematics—not used in practice!

UNDERGROUND ELECTRIC CURRENTS IN NEW YORK CITY.

THERE is a serious and growing trouble with gas and water pipes, due to hurtful electrolytic action of underground electric currents, mainly from trolley lines. Mr. A. A. Kundson has recently reported to the American Institute of Electrical Engineers the results of an electrical survey of New York City and of the Brooklyn Bridge. He finds the conditions at the anchorages to be such as have been known to do serious hurt to water pipes, although, as he points out, the action upon the massive iron anchors to which the suspending cables are attached may be very slight, because of their being surrounded by concrete in which there is a certain amount of free lime and a definite lack of those chemical salts which conduce to destructive electrolytic action. The matter is, however, sufficiently serious to be taken into deliberate consideration, as it is unlikely that the anchors can withstand the present action for a long series of years.

HIGH-VOLTAGE POWER TRANSMISSION.

MR. CHAS. F. SCOTT has recently presented to the American Institute of Electrical Engineers the results of some unique tests of high voltage transmission lines. These tests have been made partly in the laboratory by Mr. Scott and partly upon the operating plant at Telluride, Colorado, by Mr. Mershon. One of the most interesting of the results is that the loss of power due to discharge between the two wires (outgoing and returning wires) begins to be excessive when the e. m. f. reaches about 50,000 to 60,000 volts. This kind of loss was one of the uncertainties which confronted the engineers who installed the now classical plant which transmitted power (at 30,000 volts) from Schaffhausen to the Electrical Exhibition at Frankfort in 1891, and these tests of Mr. Scott are the first to show just when this loss becomes considerable under practical conditions. The highest e. m. f. at present used in power transmission is 40,000 volts at the Provo plant of the Telluride Power Transmission Company in Utah, which transmits power to a distance of thirty-five miles.

W. S. F.

NOTES ON INORGANIC CHEMISTRY.

THE current number of *Nature* contains a full abstract of a paper read by Professor W. C. Roberts-Austen at the Institution of Civil Engineers on the extraction of nickel from its ores by the Mond process. This process is an entirely new departure in

metallurgical practice and also in the principles thus far applied. It depends upon the fact that nickel forms a volatile compound, Ni $(CO)_4$, by direct union with carbon monoxid, and that this compound is decomposed with deposition of metallic nickel at 180°. The compound was discovered by Dr. Ludwig Mond in course of an attempt to eliminate carbon monoxid from gases containing hydrogen. The only other metal forming similar volatile compounds with carbon monoxid is iron; hence it seemed possible to utilize the reaction for the practical separation and purification of nickel. An experimental plant was erected at Smethwick, near Birmingham, in 1892, using as a source of nickel 'Bessemerised' matte. The matte, after dead roasting, contained 35% nickel, 42% copper and 2% iron. Two-fifths of the copper was extracted by sulfuric acid and marketed as sulfate. This residue, containing 51% nickel, was then reduced by water gas, care being taken not to reduce the iron. The material was then submitted to the action of carbon monoxid in a tower at a temperature not exceeding 100°, the volatile compound being passed to the reducer, where the nickel was deposited. The carbon monoxid was circulated between the tower and the reducer for a period varying from seven to fifteen days, in which time 60% of the nickel had been removed as nickel carbonyl. The residue was then returned to the first stage of the process. The nickel was deposited in the reducer, either on thin sheets of iron or on granules of ordinary commercial nickel. The product contained 99.8% nickel. This plant is now in full working operation and over 80 tons of nickel has been extracted from different kinds of matte. The conclusion is reached that this process is well able to compete with any other process in use for the production of metallic nickel. Professor Roberts-Austen pointed out that its application to the nickel ores of Sudbury, Ontario would probably contribute largely to the resources of the Dominion.

Continuing his researches on metallic lithium and calcium, Moissan finds that these metals are soluble in anhydrous ammonia, forming compounds more stable than those with the other alkalies. The substances are represented by the formulæ Li,NH_3 and $Ca,(NH_3)_4$, the ammonia being apparently analogous to water of crystallization. Both of these compounds take fire on coming in contact with the air at ordinary temperature.

The interesting investigations of H. N. Stokes on the compounds of phosphorus and nitrogen are continued in the November issue of the *American Chemical Journal*. It is found that on saponification, the series of chlorids $(PNCl_2)n$ gives rise to a corresponding series of acids $(PNO_2H_2)n$. The compounds where n is 3, 4, 5 and 6 have been formed, but when n is 7 the chlorid yields an acid of the formula $(PNO_2H_2)_7 + H_2O$. The constitution of these compounds is represented by a ring formula where the phosphorus and nitrogen alternate. This ring may thus contain 6, 8, 10 and 12 atoms, but when 14 atoms are present the limit is passed and the typical constitution is departed from. Applying the tension theory of von Baeyer regarding carbon atom rings, in which he finds that the most stable ring is that which contains five carbon atoms (the pentamethylene ring), Stokes finds that the phosphorus-nitrogen ring containing eight atoms would be most stable, and this is exactly borne out by the facts, $P_4N_4O_8H_8$ being the most stable of these acids. This analogy existing between the carbon rings and those of other elements is an interesting broadening of chemical theory.

The question as to the origin of petroleum continues to attract experimenters.

In his inaugural dissertation at Freiberg, Th. Lehmann finds that the distillation of fish remains under pressure gives rise to an oil which in its constituents shows a very close resemblance to petroleum, and hence the conclusion is drawn that petroleum deposits have arisen from the remains of sea animals. There is, however, in this work little advance on that of Engler. The well recognized fact that petroleum could have been formed in this way by no means proves that all petroleum has this origin, or that some or much has not been formed according to the theory of Mendeleef from the action of water upon metallic carbids in the deeper layers of the earth's crust.

J. L. H.

CURRENT NOTES ON ANTHROPOLOGY.

RUSSIAN ETHNOGRAPHY.

An unusually interesting article is that on the ethnography of the Slavic stock, by Professor W. Z. Ripley, in the *Popular Science Monthly* for October last. He finds a remarkably uniform type of head form among the Russians due, he believes, to the uniformity of their environment. Two contrasted psychical types, however, coexist throughout the Slavic nations—the one tall, blonde, long-skulled; the other of medium stature, swarthy, broad-skulled. Which represents the primitive Slavic type? Desperate contests, in which much ink has been shed, have been fought over this point by the learned of Europe. Professor Ripley does not shout in a clarion voice with either combatant, but 'rather inclines to believe' that more can be said in favor of the latter. 'The Slaves penetrated Russia from the southwest,' driving before them a primitive people ethnically allied to the Finns, hence of north Asiatic origin.

ARGENTINE ETHNOGRAPHY.

Under the title *Etnografia Argentina*, Sr. Felix F. Outes has published a supplement to his work on the Querandi Indians, mentioned in these notes (October 7, 1898). He repeats and defends his opinion that they belonged to the Guaycuru stock of the Chaco. His arguments do not seem to me convincing. The Querandi proper names appear to belong to an Auçanian dialect, and when they were driven from the coast they fled to the Ranqueles, who are a known branch of the Aucanian family.

In an article in the *Bollettino della Socièta Geografica Italiana*, 1897, Sr. Guido Boggiani copies and describes the singular rock inscriptions at the 'Gorgo das Pedras,' not far from Corumba, State of Matto Grosso. They are alleged to be extremely ancient, the modern Indians denying knowledge of their origin or meaning. They present familiar types of aboriginal petrographs, human foot-prints, bird foot-prints and the signs for man, etc.

THE ETHNOLOGICAL SURVEY OF CANADA.

The second report of the Committee on the Ethnological Survey of Canada, presented to the British Association last August, has been issued. It contains a brief official report of progress and an Appendix including 'Haida Stories and Beliefs,' by Professor C. Hill-Tout, and 'Customs and Habits of Earliest Settlers of Canada,' by Mr. B. Sulte. The Association now makes an appropriation for this work and it is progressing more rapidly.

Both the papers in the Appendix are valuable original contributions, though one cannot but regret to see that Professor Hill-Tout is engaged in discovering the affinities between the Salish dialects of British Columbia and the Polynesian languages. That is a step twenty years backward in linguistic science. Mr. Sulte's picture of the early settlers and their mode of life is vivid and striking.

D. G. Brinton.
University of Pennsylvania.

SCIENTIFIC NOTES AND NEWS.

AT a meeting of the Paris Academy of Sciences, on November 28th, Professor O. C. Marsh, of Yale University, was elected a correspondent in mineralogy. Forty-four votes were given to Professor Marsh and four to Professor Zittel, of Munich.

WE regret to learn that Professor George J. Brush is dangerously ill at New Haven with pneumonia. It will be remembered that Professor Brush recently resigned the directorship of the Yale Sheffield Scientific School, to take effect at the end of the present year. Like President Dwight, he resigns owing to the fact that he has about attained the age of seventy years.

QUEEN VICTORIA has appointed Professor D'Arcy Wentworth Thomson, M.A., of University College, Dundee, to be Scientific Member of the Fishery Board for Scotland, in the room of Sir John Murray, F.R.S., resigned.

THE Society for Plant Morphology and Physiology will hold its second annual meeting at 505 Shermerhorn Hall, Columbia University, Tuesday to Friday, December 27th to 30th. On Tuesday evening, from 8 to 11, a reception is to be tendered by the Torrey Botanical Club, of New York, to the members of the Society and other visiting botanists, in the rooms of the Department of Botany in Columbia University. The address of the retiring President, Dr. W. G. Farlow, is to be delivered on Wednesday at 4 p. m. The Society will unite with the American Society of Naturalists in their programs for Thursday afternoon and Wednesday and Thursday evenings.

THE local committee of the American Society of Naturalists and Affiliated Societies has, in view of the meetings next week, sent to members an announcement containing references to some of the more important institutions and collections of New York City. Since the Society of Naturalists met in New York, in 1889, the City has shown great scientific activity. Columbia University and New York University have been established on new sites, and special attention has been given to the erection and equipment of the scientific laboratories. The American Museum of Natural History—both the buildings and the collections—has been greatly enlarged and will be open especially to members on Tuesday. An Aquarium and a State Pathological Institute have been established. The Botanical and Zoological Parks, in their present unfinished condition, will be shown to members of the Societies by Director Britton and Director Hornaday on Friday.

WE regret to record the death of Dr. John Stillwell Schanck, emeritus professor of chemistry and hygiene in Princeton University, which occured at Princeton on December 16th. Dr. Schanck was born in 1817, and began the practice of medicine at Princeton in 1843. In 1847 he was made lecturer in zoology at the College, and in 1856 was elected professor of chemistry, to which the chair of natural history was added in 1869. In 1874 the professorship was limited to chemistry, and from 1885 until he was made emeritus professor, in 1892, his chair was entitled chemistry and hygiene.

WE must also note the death of Mr. George Woodroffe Goyder, late Surveyor-General of South Australia, and of M. D. Meritens, the French electrician.

THE *British Medical Journal* gives further details of the unveiling of the monument to Charcot on December 4th. Addresses were made by Professor Brouardel, who presented the monument to the city on behalf of the faculty of medicine; by M. Navarre on behalf of the Municipal Council; by Professor F. Raymond, Charcot's successor at the Saltpêtrière ; by Professor Cornil, Charcot's successor in the chair of pathology in the École de Médicine, and by M. Georges Leygnes, the French Minister of Public Instruction. The statue has been modelled in bronze by the distinguished sculptor, M. Falguière, with the collaboration of the well-known architect, M. Samson. Charcot is represented in his professorial robes standing in the act of giving a demonstration, the right hand indicating the left temporal region on the head of a dead body lying beside him, and the left making a gesture habitual to him in lecturing.

MR. A. E. SHIPLEY, of Christ's College, and MR. H. S. Cronin, of Trinity College, have been appointed to represent Cambridge Univer-

sity at the celebration of the centenary of the Imperial Military Academy of Medicine at St. Petersburg on the 30th inst.

MR. J. GRAHAM KERR, of Christ's College, Cambridge, has been awarded the Walsingham medal for his researches on *Lepidosiren*.

THE Zoological Society of London has sent Mr. John S. Budgett to Western Africa to study the fauna and to make collections for the Gardens.

THE *American Naturalist* states that Mr. C. F. Baker, of the Alabama Experiment Station, is about to start on a collecting trip to South America which will last eighteen months.

FROM the *American Geologist* we learn that Mr. Oscar H. Hershey has returned to his home at Freeport, Illinois, after a scientific expedition to the Isthmus of Panama.

THE United States Civil Service Commission announces that it is desired to establish an eligible register from which a selection may be made to fill an existing vacancy in the grade of assistant engineer, electrically qualified, at a salary of $1,000 per annum, in the custodian service at Baltimore, Md. Eligibility for appointment will be determined from an examination into each applicant's experience, ability and character as a workman, and physical qualifications for the work to be performed.

A POLAR reflecting photographic telescope, with its building and dome, is being erected for the observatory of Cambridge University and is now nearly ready for use.

WE take from *Natural Science* the following items: Dr. Jonathan Hutchinson is erecting for Selby, in Yorkshire, an Educational Museum similar to the one established by him in Hazelmere. A Museum of Natural History has been opened at King Williamstown, Cape Colony. The Sheffield Literary and Philosophical Society has presented Dr. H. C. Sorby with his portrait, by Mrs. M. L. Waller, in celebration of his fifty years' connection with the Society (1847–1897).

THE French Academy has received a legacy of 120,000 francs for the establishment of a prize that will appear somewhat curious to Anglo-Saxons. It is to be awarded for 'the most beautiful example of love and devotion between brothers and sisters.'

THE will of the late Edwin F. Knowlton gives $40,000 for the establishment of a library in West Upton, Mass. Mr. G. F. Logan, of Chicago, has given $35,000 for the erection of a library building for the Art Institute of that city. The will of the late John L. Gardner, of Boston, bequeathes $275,000 for public purposes, to take effect on the death of his wife; $100,000 is given to the Boston Society of Fine Arts, and $25,000 to the Brookline public library.

THE Museum of Anatomy and Anthropology at Cambridge has received from Professor Flinders Petrie a collection of skulls and bones secured at Hieraconopolis, representing prehistoric and the early dynastic races of Egypt.

MR. ALFRED JONES has subscribed £350 annually for the maintenance in Liverpool of a laboratory of tropical diseases. It will be under the direction of Professor Boyce.

AT the nineteenth annual meeting of the Biological Society of Washington for the election of officers, on Saturday, December 17th, the following were elected for 1899:

President, F. V. Coville; Vice-Presidents, W. H. Ashmead, B. W. Evermann, F. A. Lucas, C. W. Stiles; Recording Secretary, H. J. Webber; Corresponding Secretary, O. F. Cook; Treasurer, F. H. Knowlton; Additional Members of the Council: C. L. Marlatt, T. S. Palmer, C. L. Pollard, F. W. True, M. B. Waite.

MR. J. LARMOR has been elected President of the Cambridge Philosophical Society. The Vice-Presidents are Mr. F. Darwin, Professor Forsyth and Dr. Gaskell; and members of the Council: Mr. H. Gadow, Mr. D. Sharpe and Professor J. J. Thomson, Mr. A. Berry and Mr. Wilberforce.

AT a special meeting of the American Society of Civil Engineers on December 14th Mr. Hiram S. Maxim gave an illustrated lecture describing his experiments in artificial flight and the evolution of the automatic gun.

THE New York Society for Child Study will hold its fourth session at Syracuse on December 28th. Professor E. F. Buchner, the Secretary of the Society, will give an address upon its

work, and among the papers is one on fatigue by Dr. Smith Baker.

THE annual meeting of the Pennsylvania Forestry Association was held on December 12th at the College of Physicians and Surgeons, Philadelphia. President John Birkinbine made an address, and a report was read by the General Secretary, Dr. Joseph T. Rothrock. Dr. Rothrock said, according to the report in the Philadelphia *Ledger*, that there had been a great increase in the interest shown by the people during the last year in the work of the Association. They were apparently being educated up to an appreciation of the value of restoring the trees to the denuded hills. He also referred with gratification to the active sympathy shown by the Fish Commission and Game Protective Association, who had a mutual interest in the preservation of the forests. Referring to the three reservations authorized by the Act of Assembly, he said the land purchased by the State this year at tax sales amounted to 55,000 acres, and that he had personally examined many other tracts. Most of the tracts purchased are contiguous, but those which are not could be sold, as a rule, for more than they cost, and the proceeds used for purchasing tracts adjoining the nuclei of the three reservations.

THOSE interested in botany, whether or not they are special students of the science, are invited to become members of the Torrey Botanical Club, which meets twice monthly at the College of Pharmacy, 115 West 68th Street, New York. The program for the evening meetings of the Torrey Botannical Club during 1899 will include the following subjects:

Our Native Roses, by Dr. George N. Best.

A Comparison Between Geological Sequence and Biological Development in the Vegetable Kingdom, illustrated by lantern views, by Dr. Arthur Hollick.

Notes on the Flora of Santiago de Cuba, illustrated by lantern views, by Dr. Valery Havard, U. S. A.

Our Eastern Ferns, illustrated by lantern views, by Mrs. E. G. Britton and others.

The North American Blue-Eyed Grasses, by Mr. Eugene P. Bicknell.

The Adirondack Mountain Flora, by Mrs. Annie Morell Smith and others.

The Pine-Barren Flora of New Jersey, illustrated by lantern views, by Mr. Joseph Crawford and others.

Progress of Work on the New York Botanical Garden, illustrated, by Dr. N. L. Britton.

Sea-Weeds, illustrated by colored lantern slides, by Dr. C. C. Curtiss.

Cultivated Palms and their Cultivation, illustrated by living specimens, by Mr. H. A. Siebrecht.

The Spurges, by Mrs. Carolyn A. Creevy.

Evergreens, illustrated by lantern slides, by Mr. Samuel Henshaw.

Rocky Mountain Wild Flowers, illustrated by colored lantern slides, by Mr. Cornelius Van Brunt.

WE learn from *Natural Science* that the British Mycological Society held its second annual meeting in Dublin, September 19th–24th. Excursions were made to Howth, Powerscourt, Brackenstown near Swords, the woods of Avoca, Lucan and Dunran. These resulted in an addition of sixteen species to the fungus flora of Dublin and Wicklow; a list will appear in the December number of the *Irish Naturalist*. Dr. Plowright, President for the current year, delivered an address, discussing certain fungi figured in Cooke's 'Illustrations.' Papers were read by Messrs. Wager, Crossland, M'Weeney, Soppitt and Rea.

M. THIBEAUT, Chargé d'Affaires of France, has notified Secretary Hay that the French government is about to adopt precautionary measures against the introduction from this country of the San José scale, and that decrees will be issued prohibiting the importation of trees, shrubs and plants from the United States and requiring an inspection of all fruits, fresh and dried, at the point of landing in France. The general trade from this country to France will suffer little through this step, as the shipment of trees, plants and shrubs last year aggregated only $328 and those of fruit $40,000. The action of France was taken, it is said, only after some of the American States had adopted precautionary laws against the pest referred to.

THE *British Medical Journal* states that the annual *conversazione* of the Röntgen Society was given by the President and Mrs. Mansell Moullin, at St. Martin's Town Hall, on Monday, November 21st. An inspection of the exhibits ranged round the hall emphasized the fact—which many who commence working at the X-rays very soon realize for themselves—that to make any advance in the knowledge of

Röntgen radiations is by no means an easy matter. The present state of knowledge and practice is certainly represented by the Röntgen Society, and the exhibits represented the utmost limits to which the process has been brought; yet, setting aside improvements in practical technique, the matter has not been pushed a step further than when Röntgen gave his remarkable discovery to the world. Mr. Mackenzie Davidson showed his beautiful method of localization of foreign bodies and some really practical applications of his stereoscopic radiographs. The same worker has also solved the difficulty of photographing stone in the kidney. This he does by the ingenious method of eliminating movements due to respiration by only exposing when a recording lever shows absolute rest of the abdominal parietes, the patient holding his breath the while. Professor Silvanus P. Thompson, F.R.S., demonstrated experiments with the Tesla oscillator, and Mr. Campbell Swinton showed a collection of tubes which he has used in his researches. There were many other exhibits too numerous to mention. The results which Mr. Wimshurst has obtained with his influence machine are extremely good; the steadiness of the shadows given on a screen by twelve 20-inch plates is remarkable.

AT a meeting of the Royal Geographical Society, London, on November 28th, Mr. Charles W. Andrews read a paper entitled 'A Description of Christmas Island (Indian Ocean).' Mr. Andrews said according to the report in the London *Times* that, as it seemed desirable that a more complete examination of the island should be undertaken than had up to that time been made, Sir John Murray, in 1896, generously offered to pay the expenses of an expedition. The lecturer left England to carry out the undertaking in May, 1897. He gave a detailed account of the physical features of the island, and said that its climate was delightful. During the greater part of the year it resembled a very hot English summer tempered with sea breezes. The island was perfectly healthy, there being no marshes or stagnant pools, while there was a fair supply of good water. The whole island was covered with forest, except the spray-swept edges of the sea cliff and the vertical faces of the inland cliffs. Many creepers and ferns added to the beauty and variety of the forest scenery. The fauna was not a rich one. There were only five species of mammals, consisting of two kinds of rats, a shrew mouse and two bats. Rats swarmed everywhere and were very destructive. The reptiles were few and small. Insect life was fairly abundant. There were several species of land crabs, the most common being a little red crab living in burrows all over the island. The robber-crab was also very numerous, and if one sat down for a short time anywhere in the forest numbers could be seen approaching from all sides. They were good climbers and ascended trees in search of food. For some years Mr. Andrew Ross, brother of Mr. George Ross, the owner of Keeling Cocos Island, had been settled in Flying Fish Cove with his family and a few men from Cocos. During his stay some substantial houses had been built, well sunk, and fruit trees and cocoanut palms planted, and a small experimental plantation of coffee had also been made, the results showing that the island was well suited for coffee growing. In May last the total population was about 40.

UNIVERSITY AND EDUCATIONAL NEWS.

AT a meeting of the Board of Governors of McGill University, on December 1st, Lord Strathcona announced his intention of endowing the new Victoria College for Women, which he built at a cost of $250,000, to the amount of $1,000,000. Mr. W. C. McDonald also announced that he intended to endow the chair of history in the arts faculty. At the dedication, on December 20th, of the new chemistry and surgery building of the University, given by Mr. McDonald, knighthood was conferred upon him.

IT is expected that the New University of London will have its offices in the Imperial Institute, South Kensington, though it is possible that arrangements will be made to occupy the premises on Gower Street occupied by University College.

MR. AND MRS. L. LANSING ZABRISKIE have given $500 to Wells College for the purchase of physical apparatus.

MR. E. F. HOLDEN has given $6,000 to Syra-

cuse University, part of which has been used for the purchase of twenty-nine microscopes.

The will of the late Miss Elizabeth Rogers leaves all her property, valued at $100,000, to the Rogers Hall School for Girls at Lowell, Mass.

The sum of $50,000 has been collected in Pittsburg for the Methodist American University at Washington.

The growth of the various English university colleges as compared with the older universities is illustrated by the papers read before the Royal Society during the past three years. Professor Ramsay points out that of 252 such papers about half--122—were contributed from these colleges, leaving only half from Oxford, Cambridge, Scottish and Irish universities and private individuals. University College, London, contributed 57 papers, which apparently surpasses the number of papers from Oxford or Cambridge.

The Isaac Newton Studentship of Astronomy and the John Lucas Walker Studentship in Pathology at Cambridge University will be filled in January. Each studentship is of the annual value of £200 and is tenable for three years.

We announced last week that Professor W. C. Röntgen had been called from Würzburg to Leipzig. The vacancy, we now learn, is caused by the retirement of Dr. Gustav Weidermann, which takes place at the end of the present semester.

Dr. Klockmann, of Clausthal, has been appointed professor of mineralogy and geology in the Institute of Technology at Aix.

M. C. Sauvageau has been appointed professor of botany in the faculty of sciences at Dijon.

DISCUSSION AND CORRESPONDENCE.

ANLAGE OR PROTON?

To the Editor of Science: I have read with interest the letter from Professor Burt G. Wilder which you recently published in regard to the word 'proton.' I thoroughly agree with him that it is very desirable to have some word better than *rudiment* for the primitive beginning in the embryo out of which some future structure is to be developed. We need a good word to use in our language, as the Germans use 'anlage' in theirs.

The suggestion of *proton* by Professor Wilder and that of 'primordium' by Professor Willey are certainly interesting, and I think, if we are to rely upon the Greek to supply us with a new term, that the suggestion made by Professor Wilder is the best we have had. But against all these suggestions I have to make an objection. It seems to me that the time has now definitely passed when it can be taken as a necessity for science to derive *all* technical terms from the Greek. When we look the matter fairly in the face we must recognize that the claim of superlative availability of Greek words and Greek derivatives is a survival of the Mediæval epoch, which was itself a survival of the earlier Roman days, when the Greek language was the language of the cultured, and the language with which all educated persons were not merely acquainted, but familiar. Whether or not this is a loss or gain, the Greek language is not now familiar to our generation; it is a language quite apart from the present day. The fact which we have to meet is that a knowledge of Greek is far from universal, and that a thorough knowledge of Greek is rarely attained except by the special student of the language. After all, a word is merely a convenient combination of sounds to furnish a symbol for a certain idea, and there is no inherent psycological reason why sounds of the type used by the Greeks should alone serve to represent ideas of a scientific character. Indeed, is it not striking that the greatest scientific nation of the world habitually uses technical terms of its own language—that German scientists use German words?

Now, Germany has attained such preeminence in science that it is probable that in any department at least as much is published in German as in any other two languages. It is literally impossible to keep abreast with any special department of science without a knowledge of German, and this implies an acquaintance with German words used by Germans with a technical meaning. An acquaintance with such words is absolutely indispensable.

To return to the question of an English equiv-

alent for 'anlage.' Why should we not use the German term? Why is not the German combination of sounds as scientific as any Greek combination? Why should we turn to Greek, a language far remote from English, out of which we must manufacture the word we want, rather than turn to the kindred language of German, which contains the precise word we want, a word, moreover, which every one must understand, if he wishes to study the science of embryology? Another important consideration is that the word 'anlage' has already been so widely and so extensively adopted both in England and America that it is now probably universally understood and often looked upon by those who use it as an accepted and established English word. Indeed, I believe its usage has become so thoroughly established that not only is the word indispensable, but also it has become impracticable to attempt to modify it; that the substitution of *proton* or *primordium* could not be accomplished, and that the attempt to make such substitution would only create an unfortunate conflict of terms. I hope, therefore, that these attempts and all similar ones will be renounced. It may also be described as a nuisance—this incessant introduction of superfluous scientific terms—and particularly in the form it takes of constantly adding a great many elaborate artificial words of Greek origin which are thoroughly unnecessary for scientific purposes. It would tend far more to the promotion of science to strike out altogether—so that they should be forever forgotten—half of the terms which have been introduced during the last twenty years, than to make any further additions to them. The load of unnecessary technical terms which we have to carry is a terrible impediment, which hinders our progress every day. I cannot but consider it the result of a pedantic superstition, which appears like a Moloch to whom the life of Science must pay a heavy sacrifice.

CHARLES SEDGWICK MINOT.

HARVARD MEDICAL SCHOOL,
December 10, 1898.

IS THE BEACH PEA FOOD OR POISON?

CAN any botanical reader of SCIENCE give me any information in regard to the use as food of the seeds of the common Beach Pea (*Lathyrus maritimus*)?

Some twenty years ago I was told a story of some children near Boston who were terribly poisoned from thoughtlessly eating these peas. In fact, my impression is that one or more of them were said to have died from the effects of the poison. Recently, however, in looking over the files of the 'Meddelelser om Grønland,' I came across the following note. In Lange's 'Conspectus floræ grœnlandicæ,' under the heading *Lathyrus maritimus* (Med. om Grønl., Hefte 3, p. 233), the author writes as follows: 'Obs. Seminibus combustis ad coffeam parandam utuntur Grœnlandi,' quoting Wormsksjöld as his authority. Of course, it is possible that roasting the peas to make coffee, neutralizes the poisonous principle if there be any. I should be glad of any further information on the subject, and, at all events, wish especially to call attention to this curious substitute for coffee.

JOHN MURDOCH.

PUBLIC LIBRARY, BOSTON.

[WE are informed by Mr. V. K. Chesnut that the seed of the beach pea, *Lathyrus maritimus*, is eaten while still green in a number of places, including England, parts of Alaska, Kamchatka and the Island of Yeso. No recorded cases of poisoning from this species are known to him, but a peculiar kind of poisoning is caused by eating the seeds of other species of *Lathyrus* for long periods at a time. It is a curious disease which at one time and another has been very prevalent in India. Horses have recently been killed there by eating imported seeds of some of the vetches. It seems possible, therefore, that some toxic principle may be present in the beach pea. The question might be settled if a series of experiments were made on guinea pigs.—EDITOR SCIENCE.]

SCIENTIFIC LITERATURE.

Encyklopaedie der mathematischen Wissenschaften. Mit Unterstützung der Akademieen der Wissenschaften zu München und Wien und der Gesellschaft der Wissenschaften zu Göttin-

gen, herausgegeben von H. Burkhardt und W. F. Meyer. Leipzig, Teubner. 1898. Band I. Heft 1. Pp. 112.

This is an undertaking of extraordinary importance and promise.

Its aim is to give a consecutive presentation of the assured results of the mathematical sciences in their present form, while, by careful and copious references to the literature, giving full indications regarding the historic development of mathematical methods since the beginning of the nineteenth century. The work begins with twenty-seven pages on the foundations of arithmetic by Hermann Schubert, of Hamburg. Schubert's reputation was made by his remarkable book on enumerative geometry. He has since applied the modern ideas in an elementary arithmetic, and is known in America as a contributor to the *Monist*.

Unfortunately Schubert has made in public some strange slips. In an article, 'On the nature of mathematical knowledge,' in the *Monist*, Vol. 6, p. 295, he says: "Let me recall the controversy which has been waged in this century regarding the eleventh axiom of Euclid, that only one line can be drawn through a point parallel to another straight line. The discussion merely touched the question whether the axiom was capable of demonstration solely by means of the other propositions or whether it was not a special property, apprehensible only by sense-experience, of that space of three dimensions in which the organic world has been produced, and which, therefore, is of all others alone within the reach of our powers of representation. The truth of the last supposition affects in no respect the correctness of the axiom, but simply assigns to it, in an epistemological regard, a different status from what it would have if it were demonstrable, as was one time thought, without the aid of the senses, and solely by the other propositions of mathematics."

If Schubert had written this seventy-five years ago it might have been pardonable. Just at the beginning of this century Gauss was trying to prove this Euclidean parallel-postulate. Even up to 1824 he was in Schubert's state of mind, for he then writes Taurinus: "Ich habe daher wohl zuweilen *im Scherz* den Wunsch geäussert, dass die Euclidische Geometrie nicht die Wahre wäre."

But the joke had even then gone out of the matter if Gauss had but known it, for in 1823 Bolyai Janos had written to his father: "From nothing I have created a wholly new world."

Of the geometry of this world as given also by Lobachévski, Clifford wrote: "It is quite simple, merely Euclid without the vicious assumption."

But this assumption is only vicious if supposed to be 'apprehensible by sense-experience' or 'demonstrable by the aid of the senses.'

That 'the organic world has been produced' in Euclidean space can never be demonstrated in any way whatsoever. On the other hand, the mechanics of actual bodies might be shown by merely approximate methods to be non-Euclidean. Therefore, Schubert's contribution on the foundations of arithmetic may fairly be read critically. He begins with counting, and defines number as the result of counting. This is in accord with the theory that their laws alone define mathematical operations, and the operations define the various kinds of number as their symbolic outcome.

There is no word of the primitive number-idea, which is essentially prior to counting and necessary to explain the cause and aim of counting. This primitive number-idea is a creation of the human mind, for it only pertains to certain other creations of the human mind which I call artificial individuals. The world we consciously perceive, is a mental phenomenon. Yet certain separable or distinct things or primitive individuals, we cannot well help believing to subsist somehow 'in nature' as well as in conscious perception. Now, by taking together certain of these permanently distinct things or natural individuals, the human mind makes an artificial individual, a conceptual unity.

Number is primarily a quality of such an artificial individual.

The operation of counting was made to apply to such an individual to identify it with one of a standard set of such artificial individuals, and so to get the exact shade of its numeric quality. These standard individuals were primarily sets of fingers. Then came the written

standard set, *e. g.*, III., or 1+1+1; and finally the written symbol, 3. Such symbols serve to represent and convey the numeric quality. The word 'number' is applied indiscriminately to the quality or idea and to its symbol.

Schubert tells us that in antiquity the Romans represented the numbers from one to nine by rows of strokes, as 4 is still represented on our watches; while the Aztecs used to put together single circles for the numbers from one to nineteen. I have seen Japanese use columns of circles in the same way. Thus, also, our striking clocks convey a numeric quality by a group possessing it. But the number pertaining to a group or artificial individual is far from being the simple notion it seems. If numbers are used to express exactly this definite attribute of finite systems they are called cardinal numbers.

Schubert's first sentence is: "Dinge *zählen* heisst, sie als gleichartig ansehen, zusammen auffassen, und ihnen einzeln andere Dinge zuordnen, die man auch als gleichartig ansieht." This may be rendered: "*To count* things means to consider them as alike, to take them together, and to associate them singly to other things which one also considers as alike." I would prefer to say: "To count distinct things means to make of them an artificial individual or group, and then to identify its elements with those of a familiar group." When the mind of man made these artificial individuals they were found to possess a sort of property or quality which was independent of the distinctive marks of the natural individuals composing them, also independent of the order or sub-association of these natural individuals. Whether the artificial individual were made of a church, a noise and a pain, or made of three peas, or composed of two eyes and a nose, it had one certain quality—it was a triplet.

I see no necessity for Schubert to consider the church as like the noise and the pain. Again, the individuals of the familiar group used in the count need not be alike. Even the individuals used by a clock in counting differ ordinally, and when we follow the count of the clock we use words all different. The primitive written number is such a picture of a group of individuals as represents their individual existence and nothing more, *e. g.*, III.; so, however different they may be, this number is independent of the order in which they are associated with its elements.

Schubert wastes three sentences on the so-called concrete number, *benannte* Zahl. Three quails is not a number, but is a particular bevy. His Section 2, *Addition*, he begins thus: "If one has two groups of units such that not only all units of each group are alike, but that also each unit of the one group is like each unit of the other group," etc. All this likeness and alikeness seems unnecessary. Any two groups may be thought into one group. Any two primitive numbers may be added.

In Section 5, Peacock's Principle of Permanence is given in Hankel's general form: The combination of two numbers by any defined operation is a number, such that the combination may be handled as if it gave one of the previously defined numbers. New kinds of numbers, like all numbers, are defined by the operations from which they result. Thus are introduced zero and negative numbers, and later the fraction. After this all is easy to the end of Schubert's contribution.

It only remains to point out, as of especial importance, that from beginning to end not the slightest mention is made of measurement. Not a word is wasted on people who do not clearly see that number is long prior to measurement.

The second section of the *Encyklopaedie* is 'Kombinatorik,' by E. Netto. This is a part of mathematics which never fulfilled the hopes of the school which was lost in it during the early part of this century. Of the most comprehensive monographs the last two are in 1826 and 1837. For us it has gone over into determinants, and more than half of Netto's article is devoted to determinants. This article is particularly valuable from a bibliographic and historic point of view.

The third section is 'Irrationalzahlen und Konvergenz unendlicher Prozesse,' by A. Pringsheim. It begins on page 47, and goes past the end of the Heft. This is a modern subject, of intense living interest. How entirely modern it is might not be suspected by readers of such sentences in Cajori's excellent history of mathematics as those on page 70: "The first incommensurable ratio known seems to have been

that of the side of a square to its diagonal, as 1 : $\sqrt{2}$. Theodorus of Cyrene added to this the fact that the sides of squares represented in length by $\sqrt{3}$, $\sqrt{5}$, etc., up to $\sqrt{17}$, and Theætetus, that the sides of any square, represented by a surd, are incommensurable with the linear unit." Now in fact Theodorus and Theætetus made no representation whatever of the length of these sides, simply saying, *e. g.*, that the side of the square whose area is 3 is incommensurable with the side of the square whose area is one. For Euclid there was no such ratio as 1 : $\sqrt{2}$; for 1 is a number and so if it could have had a ratio to $\sqrt{2}$ this would have been a number. But Euclid, Book X., Proposition 7 is: "Incommensurable magnitudes are not to one another in the ratio of one number to another number."

The Hindus were the first to recognize the existence of irrational numbers. Even through the Middle Ages and the Renaissance they were absurd fictions, 'numeri surdi,' a designation attributed to Leonardo of Pisa. The first writer to treat them genuinely was Stifel (1544), and even he had not completely freed himself from the older terminology, since he says: "sic irrationalis numerus non est verus numerus atque lateat sub quadam infinitatis nebula."

In reference to the next step, the conceiving of ratio as number, Pringsheim says, page 51: "Hatte schon *Descartes* beliebige *Streckenverhältnisse* mit *einfachen Buchstaben* bezeichnet, und damit *wie mit Zahlen* gerechnet," etc. But here I think the careful German has slipped.

In regard to just this point a common error is still widespread, which we see in the following, read before Sections A and B of the American Association for the Advancement of Science, 1891:

"The doctrine of Descartes was that the algebraic symbol did not represent a concrete magnitude, but a mere number or ratio, expressing the relation of the magnitude to some unit. Hence that the product of two quantities is the product of ratios, * * * ; that the powers of a quantity are ratios like the quantity itself," etc.

That every statement here quoted is a mistake will be instantly seen from the following, taken from pages numbered 297–9 of the *original edition* of Descartes' Geometrie, 1637, a copy of which (perhaps unique on this continent) I have had the good fortune to possess since my student days (1876).

"Et comme toute l'Arithmetique n'est composée que de quatre ou cinq operations, que sont l'Addition, la Soustraction, la Multiplication, la Diuision, & l'Extraction des racines, qu'on peut prendre pour vne espece de Diuision : Ainsi n' at' on autre chose a faire en Geometrie touchant les lignes qu'on cherche, pour les preparer a estre connues, que leur en adiouster d'autres, ou en oster; Oubien en ayant vne, que ie nommeray l'vnité pour la rapporter d'autant mieux aux nombres, & qui peut ordinairement estre prise a discretion, puis en ayant encore deux autres, en trouuer vne quatriesme, qui soit a l'vne de ces deux, comme l'autre est a l'vnité, ce qui est le mesme que la Multiplication; oubien en trouuer vne quatriesme, qui soit a l'vne de ces deux, comme l'vnité est a l'autre, ce qui est le mesme que la Diuision ; ou enfin trouuer vne, ou deux, ou plusieurs moyennes proportionnelles entre l'vnité, & quelque autre ligne ; ce qui est le mesme que tirer la racine quarrée, ou cubique, &c. Et ie ne craindray pas d'introduire ces termes d'Arithmetique en la Geometrie, affin de me rendre plus intelligible. * * *

"Mais souuent on n'a pas besioin de tracer ainsi ces lignes sur le papier, & il suffist de les designer par quelques lettres, chascune par vne seule. Comme pour adiouster la ligne BD a GH, ie nomme l'vne a & l' autre b, & escris $a+b$; Et $a-b$, pour soustraire b d' a; Et ab, pour les multiplier l'vne par l' autre ; Et $\frac{a}{b}$, pour diuiser a par b; Et aa, ou a^2, pour multiplier a par soy mesme ; Et a^3, pour le multiplier encore vne fois par a, & ainsi a l' infini ; Et $\sqrt{a^2+b^2}$, pour tirer la racine quarrée d' a^2+b^2; Et $\sqrt{C.\, a^3-b^3+abb}$, pour tirer la racine cubique d' a^3-b^3+abb, & ainsi des autres.

"Ou il est a remarquer que par a^2 ou b^3 ou semblables, ie ne concoy ordinairement que des lignes toutes simples, encore que pour me seruir des noms vsités en l' Algebre, ie les nomme des quarrés, ou des cubes, &c."

Thus what Descartes really did was to make a geometric algebra, in which, however, the product of two *sects* (Strecken) was not a rectangle but a sect; the product of three sects not a cuboid but a sect. Here Descartes represents by the single letters a, b, sects, Strecken, not *Streckenverhältnisse*. Descartes does not here pass beyond Euclid's representation of the ratio of two magnitudes by two other magnitudes, does not reach the conception of the systematic representation of the ratio of two magnitudes

by one magnitude, that one magnitude to be always interpreted as a number. This radical innovation, the creation of this epoch-marking paradox, is due to Newton. Newton takes this vast step explicitly and consciously. The lectures which he delivered as Lucasian professor at Cambridge were published under the title, 'Arithmetica Universalis.' At the beginning of his Arithmetica Universalis he says, page 2: "Per Numerum non tam multitudinem unitatum quam abstractam quantitatis cujusvis ad aliam ejusdem generis quantitatem quae pro unitate habetur rationem intelligimus." [In quoting this, Pringsheim, p. 51, misses the first word. He omits the *Per*.]

As Wolf puts it (1710): "Number is that which is to unity as a piece of a straight line [a sect] is to a certain other sect." Thus the length of any sect is a real number, and the length of any possible sect incommensurable with the unit sect is an irrational number.

Says Hayward in his Vector Algebra (1892), page 5: "Number is essentially *discrete* or *discontinuous*, proceeding from one value to the next by a finite increment or jump, and so cannot, except in the way of a limit, represent, relatively to a given unit, a continuous magnitude for which the passage from one value to another may always be conceived as a *growth* through every intermediate value."

But the moment we accept Newton's definition of number it takes on whatever continuity is possessed by the sect. However, from this alone does not follow that for every irrational there is a sect whose length would give that irrational. G. Cantor was the first to bring out sharply that this is neither self-evident nor demonstrable, but involves an essential pure geometric assumption.

To free the foundations of general arithmetic from such *geometric* assumption, G. Cantor and Dedekind each developed his pure arithmetic theory of the irrational.

Professor Fine, in his 'Number-System of Algebra,' seems to miss this point completely. He gives, page 42, what purports to be a demonstration that "corresponding to every real number is a point on the line, the distance of which from the null-point is represented by the number," without any mention of the geometric assumption necessary, and then proceeds, page 43, to borrow the continuity of his number system from the naïvely supposed continuity of the line, the very thing for the avoidance of which G. Cantor and Dedekink made their systems.

Says Dedekind. "Um so schöner erscheint es mir, dass der Mensch ohne jede Vorstellung von messbaren Grössen, und zwar durch ein endliches System einfacher Denkschritte sich sur Schöpfung des reinen, stetigen Zahlenreiches aufschwingen kann; und erst mit diesem Hülfsmittel wird es ihm nach meiner Ansicht möglich, die Vorstellung von stetigen Raume zu einer deutlichen auszubilden."

GEORGE BRUCE HALSTED.

AUSTIN, TEXAS.

The Tides and Kindred Phenomena in the Solar System. By GEORGE HOWARD DARWIN. Boston and New York, Houghton, Mifflin & Co. 1898. Pp. xviii + 378.

During October and November, 1897, Professor Darwin delivered a course of semi-popular lectures on tidal phenomena at the Lowell Institute, Boston, Massachusetts. Since then the author has prepared these lectures for the press, and they are now, through the enterprise of Messrs. Houghton, Mifflin & Co., placed before the reading public in attractive book form.

The salient features of oceanic tides are more or less familiar to most people in these days. Indeed, some intelligent people will tell us that it is only necessary to read the daily papers of the seaboard towns, or to look in 'The Farmers' Almanac,' to learn when high and low water will occur. The educated public was not always so well informed, however. When, for example, Alexander the Great attempted to make a landing at the mouth of the Indus his fleet was nearly overwhelmed by the inrush of the tide. "The nature of the ocean," according to his biographer, Curtius, "was unknown to the multitude, and grave portents and evidences of the wrath of the gods were seen in what happened." The admirals of the present day know more about tides than the admiral of Alexander, and the wrath of a court of enquiry, rather than the wrath of the gods, hangs over the head of any commander who exposes his fleet to tidal dangers. But whence comes

the knowledge that enables us to anticipate the rise and fall of the ocean? How are the tidal tables of the daily papers and of 'The Farmers' Almanac' constructed?

It was the primary object of the lectures of Professor Darwin to answer such questions; to explain in a popular way, without the aid of mathematical and physical technicalities, how, from observations of the tides and from the modern theory thereof, predictions of the rise and fall of the ocean at any port may be issued years in advance. In addition to these more obvious tidal phenomena he has also discussed the more recondite phenomena of bodily tides in the earth and other members of the solar system. Thus, from questions of commercial or otherwise purely practical significance, the reader is led up to questions in cosmology of the highest scientific importance, especially in their bearings on the remote history, past and future, of our planet.

The task which Professor Darwin set for himself was a difficult one. Few, if any, questions in the mathematico-physical sciences are more profoundly complicated than those presented by tidal phenomena. Their elucidation has taxed the ingenuity of the most laborious investigators from the time of Newton to the present day. In the highly condensed language of mechanics it may be said that these phenomena, in any case, are simply the outcome of the energy, the angular momentum and the friction involved. But to turn conclusions expressed in such language into common parlance would seem to be almost as great a work as that of reaching the conclusions themselves. No one less well equipped than Professor Darwin would have dared to undertake this task. Thoroughly familiar with the details of tidal action, and himself a principal contributor to recent advances in tidal theories, he has produced a charmingly interesting and instructive book, which may be read with profit by those who know much as well as by those who know little of the tides.

The book is divided into twenty chapters under the following titles: Tides and Methods of Observation, Seiches in Lakes, Tides in Rivers—Tide Mills, Historical Sketch, Tide-generating Force, Deflection of the Vertical, The Elastic Distortion of the Earth's Surface by varying Loads, Equilibrium Theory of Tides, Dynamical Theory of the Tide Wave, Tides in Lakes—Cotidal Chart, Harmonic Analysis of the Tide, Reduction of Tidal Observations, Tide Tables, The Degree of Accuracy of Tidal Prediction, Chandler's Nutation—The Rigidity of the Earth, Tidal Friction, Tidal Friction (continued), The Figures of Equilibrium of a Rotating Mass of Liquid, The Evolution of Celestial Systems, Saturn's Rings. Each chapter is followed by a list of authorities on the subject of the chapter, and a good index completes the volume.

R. S. W.

The Elements of Physics. By ALFRED PAYSON GAGE. Boston, Ginn & Co. 1898. 12mo. Pp. x+381.

The author of this book put forth his first edition sixteen years ago and has long been favorably known as a reliable authority in the school room. The motto then adopted, 'Read Nature in the Language of Experiment,' is very properly retained in the present volume, which is not a revision, but a new book differing quite radically from the first in its method of presentation. The change, moreover, is a great improvement. We all agree that the experimental method is the proper method of investigating what is collectively called Nature, but there has been much difference of opinion about the advisability of regarding elementary pupils in the high school as fit to acquire their fundamental conceptions of physics by independent discovery. In the preface to the present volume Dr. Gage repeats the expression of his belief in the importance of the laboratory method in the high school, but adds that he has 'observed the development of a tendency which threatens seriously to impair its usefulness.' He is now 'convinced that both mental discipline and the acquisition of knowledge will be promoted if theory and experiment be somewhat sharply divided.'

There are a good many of us who have long held this last view in opposition to that which was carried out in Dr. Gage's first book. The demand for laboratory methods in the school room is much more than sixteen years old. A protest against the abuse of them was distinctly

formulated by Dr. Mendenhall in his vice-presidential address delivered before the Section of Physics at the Montreal meeting of the American Association for the Advancement of Science in 1882, the very year when Dr. Gage's book appeared. It is probably safe to say that a majority of American teachers of physics are now agreed in the view that the elementary pupil should not enter the laboratory as an original investigator, because he is utterly unfit to be such until after much training has been received. What he needs is a well arranged, clear and accurate presentation of principles, with such experimental demonstrations by the teacher as may be needed to ensure the acquisition of the truth. After a good introduction has thus been received he should have the opportunity to make a selected series of tests of these principles in the laboratory, and advantage should be taken of such practice to train him into habits of close observation, system, neatness and good order. He needs, therefore: first, a reliable class text-book, the study of which should accompany the teacher's lectures; and, second, a separate laboratory manual, or its equivalent in the form of special written or printed instruction cards adapted to the particular apparatus that is put into his hands after the requisite class-room preparation has been secured. Should he in time manifest enough originality to become an investigator, his work will probably be amid surroundings better adapted for research than the school laboratory.

It is to meet the first of these needs, a reliable class text-book, that the present volume has been written. The author exhibits good judgment, not only in the selection of what he includes, but in omitting certain special topics, such as the polarization of light, of which a smattering is often unwisely given. Such subjects as absorption, osmose and crystallization belong now to the newly differentiated science of physical chemistry. Nevertheless, there remain some embarrassments due to the necessity to avoid mathematics, and the attempt to be a trifle too conservative by recognizing certain details of nomenclature that are deservedly passing away. For example, the poundal is recognized as a unit of force. The only excuse for the invention of such a unit has been to bring the clumsy 'British' system into accordance with the far simpler system, employed by all physicists irrespective of nationalty, that for the sake of contrast is often called the absolute system. Engineers in England and America express weights in pounds, but they have no use for poundals in either theory or practice. The physicist thinks in the simpler system, but often has to translate his final results into the British system. There is hence no use in befogging the minds of young pupils with more than one unit of force, the dyne. Nor is any advantage to be derived from specifying two methods of measuring force, calling one the statical or gravitational system, and the other the dynamical or absolute system. The latter is the only one needed; the former is always expressible in terms of the absolute system. The suggestion of duality is confusing and often misleading. The confusion culminates where the pupil is confronted with poundal, foot-poundal, pound, foot-pound, dyne, erg, gram, gram-centimeter and kilogram, all grouped in a single diagram (p. 71) for the purpose of contrasting the units of force and of work. If all calculations are made in terms of centimeters and grams there is but little trouble in translating final results if necessary.

The author in similar manner speaks of density, specific density and specific gravity. Of these three terms the first is the only one that is really needful, though it may sometimes be convenient to employ the term specific gravity, or relative density, to denote that a secondary rather than primary standard is employed. The distinction between specific density and specific gravity is, of course, definable, but in the interest of simplicity it is not desirable.

In illustrating any subject with numerical examples it is best to employ such as are approximately within the range of practice. After a good description of Joule's apparatus for the determination of the mechanical equivalent of heat, which was operated in Joule's laboratory, an example is given in which the weights are supposed to be raised to a height exceeding that of an ordinary church steeple. There is, of course, no theoretical objection to this. But it is practically of some importance that the final

result should be given in the corrected form which was accepted by Joule after Rowland's exhaustive experiments in Baltimore. Joule's equivalent is now quite generally quoted as 427 rather than 424 kilogram-meters at ordinary laboratory temperature.

The difficulty of conveying clear ideas without mathematical methods is particularly felt in the attempt to define elasticity and to employ this word intelligibly in the formula for the velocity of propagation of a wave. To say that this velocity varies as the square root of the quotient of elasticity by density conveys no idea, unless modulus of elasticity has been previously defined and abundantly illustrated. The ordinary student regards india-rubber, a highly compressible solid, as the type of elasticity, while in reality its modulus of elasticity is exceptionally small. The stretch modulus is defined in an appendix to the present volume; but it is not concerned in the propagation of a sound wave through air, and it is in this connection that the formula is given. To account for the high velocity of sound in solids and liquids by reference to their superior incompressibility is inadequate unless the relation between compressibility and the volume modulus of elasticity has been already made plain. The elementary student has scarcely any alternative but to memorize words in this connection, and to trust to the future for the ideas they are intended to convey, however faithful the author of the text-book may have been to put into English what is always beyond the youthful reader.

The presentation of the subject of electric potential is unusually well given; it is, indeed, as good as could possibly be expected without mathematics. The general treatment of electricity is clear and up to date, several pages being devoted to X-rays and the phenomena of alternating currents of high potential and high frequency.

The book is not free from typographical errors, but these are in no case serious. There are occasional statements of minor importance to which exception may be taken, but the author is generally accurate and reliable, and his skill in the art of presentation is unquestioned. Among the welcome features are woodcut reproductions of the portraits of Archimedes, Galileo, Newton, Franklin, Faraday and Lord Kelvin.

W. Le Conte Stevens.

Washington and Lee University,
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Leçons de chimie physique. Professées a L'Université de Berlin. Par J. H. Van't Hoff, Membre de l'Académie des Sciences de Berlin, Professeur ordinaire a L'Université, et Directeur de L'institut de Physique de Charlottenburg. Translated from the German by M. Corvisy, Professeur agrégé au Lycée de Saint-Omer. Première partie. *La Dynamique Chimique.* Librairie Scientifique. Paris, A. Hermann. 1898.

This work, as the title implies, is a translation of Van't Hoff's 'Vorlesungen über theoretische und physikalishe Chemie,' or of that part of it which has thus far appeared—Chemical Dynamics. The book, as the author states, "is based upon the lectures which I give at the University of Berlin, on 'Selected Chapters in Physical Chemistry.' Indeed, it contains more than these lectures, since, in the limited time at my disposal, I was able to take up only some of the more important points, in order to cover the entire field in one lecture a week during four semesters." The method of treatment is that adopted by Lothar Meyer in the later editions of his 'Modern Theories of Chemistry.' The whole subject is treated under the general heads of Statics and Dynamics; Statics dealing with homogeneous substances, with views as to the structure of matter, with the molecular and atomic conception, and with the determination of constitution; Dynamics, with the reciprocal transformation of several substances, with affinity, reaction, velocity and chemical equilibrium. A third part is added, on the relation between physical and chemical properties and composition.

The order is, however, reversed. Chemical Dynamics, having been placed on a surer basis by thermodynamics, has acquired greater prominence, and is dealt with at the beginning. We have then: First, *Chemical Dynamics*; second, *Chemical Statics*, and third, *Relations between Properties and Composition.*

The advantage of this order is that in the first part of the work only the molecular conception enters, while the atomic hypothesis and the problem of configuration do not appear

until the second, and the problem of the relations of substance to substance, about which we still know very little, is relegated to the third and last division.

The first part, or chemical dynamics, which is now available in German and in French, treats the subject under the two general heads of *Chemical Equilibrium* and *Reaction Velocity.* We have the physical and chemical equilibria in a homogeneous substance, between two substances, between three substances, between four substances; chemical equilibrium from the molecular-mechanical standpoint; homogeneous and heterogeneous equilibra; the law of reaction velocity; reaction velocity and equilibrium; reaction velocity and affinity; mono-, bi- and tri-molecular reactions; effect of the surroundings and medium on reaction velocity; effect of temperature on the reaction velocity; effect of pressure on the reaction velocity.

The translation of this, a part of Van't Hoff's work, before the appearance of the remainder, is indicative of that esteem in which he is so justly held, not only at home, but in foreign lands. The translation into French seems to have been very carefully done, and the French edition is an inviting one, barring an occasional typographical error.

It is a matter of delight to all who are interested in physical chemistry that books are appearing simultaneously on the same chapter of their subject, from the pens of two of the great leaders in this field of work. As is well known, that portion of Ostwald's Lehrbuch which deals with the broad subject of *Verwandtschaftslehre* is now available in part. These two works admirably illustrate the difference in method of these two master minds, and each is enhanced in value by the other.

HARRY C. JONES.

Laboratory Directions for Beginners in Bacteriology. By VERANUS A. MOORE.

This book of ninety pages contains the outlines of an introductory laboratory course divided into sixty lessons, and aims to impart a technical and working knowledge of the more essential bacteriological methods and to develop a definite knowledge of a few important species of bacteria. The book is not intended to replace the text-book on bacteriology, but to be a manual for use at the laboratory desk in which through a series of carefully selected exercises the student, without waste of time, will cover the necessary ground.

A manual such as this represents strongly in its selections and in the amount of time alotted to each subject the personal opinions of its author, yet we believe on the whole the judgment of the writer will be approved by teachers.

This book will be found very useful by teachers who have not the time to prepare and print their own outlines. Even those who are compelled to give a course much shorter than that sketched in this book can easily, without serious harm, reduce the length of the course by omitting the practical work in some of the chapters and shortening it in others. The classification of the bacteria upon the system of Migula seems to us a mistake, for it necessitates many changes in the accustomed nomenclature; thus the name bacillus is limited to motile rod-shaped organisms to which the flagella are attached to all parts of the body. A bacillus with polar flagella becomes a pseudo-monas and one without any flagella a bacterium. As this book is intended to be used along with various text-books on bacteriology, it would seem wiser to have omitted any elaborate and unusual classification which, however valuable, must of necessity frequently clash with that used in the text-book, and thus tend to confuse the student.

WM. H. PARK.

GENERAL.

THE U. S. Department of Agriculture has issued a bulletin on Fish as Food (*Farmers' Bulletin*, No. 85), by Dr. C. F. Langworthy, of the Office of Experiment Stations, in which the results of investigations on the nutritive value of various kinds of sea food have been summed up for the general reader. The chemical composition of a considerable number of fresh and preserved fishes, mollusks, crustaceans, etc., are given; the relative cost of protein and energy in fish and other food material is shown; the place of fish in the diet is discussed, and some sample menus are given to show how fish may be combined with other food materials to make a well-balanced dietary. The popular notion

that fish is a 'brain food' is combatted, but it is stated that 'most physiologists regard fish as a particularly desirable food for persons of sedentary habits.'

THE second and third volumes of Jordan and Evermann's 'Synopsis of the Fishes of North and Middle America' have appeared, but the volume of illustrations, it is understood, may be delayed for some months. When the last volume is published, a review may be expected in SCIENCE.

THE new 'Life of Michael Faraday,' by Professor Silvanus Thompson, which Messrs. Cassell & Co. will publish shortly, contains, says *Literature*, many points that have not appeared in any earlier biography. Several hitherto unpublished letters and a poem by Faraday himself are included, as well as a number of extracts from his laboratory note-books, from which also some sketches of apparatus are reproduced in facsimile. Fresh light is thrown upon Faraday's refusal, in 1836, of the pension offered him by Lord Melbourne.

SCIENTIFIC JOURNALS.

THE *American Naturalist* for December opens with an article by Mr. L. P. Gratacap of the American Museum of Natural History on the Relations of James Hall to American Geology and a portrait of Dr. Hall is given as a frontispiece. The work on the Wings of Insects by Professor Comstock and Dr. Needham is continued. Professor J. L. Howe contributes an interesting article on variation in the shell of *Helix Nemoralis* in the Lexington (Virginia) colony. Mr. H. H. Field describes the work of the *Concilium Bibliographicum*. He states that it has been conducted at a considerable loss, but that its future is now assured by the subsidy voted to it by the Swiss Confederation, the Canton and the town of Zurich. It is said that, while South America and Hawaii have ordered several complete sets of the cards, there is only one such set in New England. The last article of the number is by Mr. O. P. Hay on 'Protostega, the systematic position of Dermochelys, and the morphogeny of the Chelonian Carapace and Plastron.' We regret to see that Dr. Robert P. Bigelow feels compelled to resign his position as editor-in-chief of the *Naturalist*, as he is unable to devote to it the large amount of time required for its management.

THE *American Geologist*, for December, contains the following articles: 'On the Dikes in the Vicinity of Portland, Maine,' E. C. E. Lord; 'Thomsonite and Lintonite from the North Shore of Lake Superior,' N. H. Winchell; 'Primitive Man in the Somme Valley,' Warren Upham; 'The Great Terrace of the Columbia and other Topographic Features in the Neighborhood of Lake Chelan, Washington,' Israel C. Russell; 'The Occurrence of Cretaceous Fossils in the Eocene of Maryland,' Rufus Mather Bagg, Jr.

THE *Biologische Centralblatt* issued on October 27, 1898, contains a memorial notice of the late Professor Theodore Eimer by his former assistant, Gräfin Dr. Maria von Linden. It may be remembered that Dr. von Linden contributed to this JOURNAL (Vol. IV., p. 308) an account of Eimer's work in certain directions.

WE have received the first number of the *L' Intermédiare des neurologistes et des aliénistes*, edited by the competent neurologist, M. Paul Sollier, and published by M. Felix Alcan, Paris. A most curious feature of the journal is the publication of its contents in French, German and English versions, a plan that would scarcely occur to a German or an Englishman. It is no wonder that under these conditions the editor asks that, considering the space required by 'the threefold texte,' correspondents are requested to write 'in the most possible short manner.' It would, we feel sure, be interesting to quote in full the editorial introduction, but we have only space for the concluding sentences: "The interest of informations taken and given, the pleasure of exposing personal opinions on subjects of high importance with the hope of being useful to others equally interested to them will, I hope, be sufficient movus to permit us to expect a collaboration which will find us very grateful. It will depend of them to whom we address that this organ, modest at its beginnings, should take, in the course of time, more and more importance and more extent, and we pray for some credit before any judgment."

SOCIETIES AND ACADEMIES.

ENTOMOLOGICAL SOCIETY OF WASHINGTON, DECEMBER 1, 1889.

UNDER the head of election of officers all of the officers serving during 1898 were re-elected for the year 1899. E. Dwight Sanderson, of College Park, Maryland, was elected an active member.

Under the head of exhibition of specimens and short notes, Mr. Schwarz spoke of the Scolytidæ of Arizona, showing that thirty two species had been collected in southern Arizona by Mr. Hubbard and himself. Nineteen of these species occurred in the pine region ; seven in the oak zone, and six in the lowest region. Of the whole number, only ten species proved to be identical with previously described forms, and all but one of the identified species belong to the pine regions.

Dr. A. D. Hopkins, by invitation of the Chair, presented some notes on Scolytidæ with especial reference to habits. He showed the male of *Hypothenomus*, which is relatively of extremely small size and is rare. *H. dissimilis* breeds in dead twigs where the larvæ consume a kind of ambrosia. In the first lot of eggs deposited only one is a male and this apparently is the only male in the number of successive broods developed as a product of a single female. There is, therefore, intense polygamy and in-and-in breeding. Further notes were given on the habits of *Cnesinus strigicollis* and *Pityophthorus minutissimus*. He showed that the larvæ of Scolytids are sometimes killed by not very severe freezing, from which he considered that the remarkable disappearance of *Dendroctonus frontalis* in 1893 was due to the severe freeze of 1892–93. He mentioned a stridulating sound made by *Dendroctonus terebrans* by rubbing the dorsal margin of the last abdominal segment against the inner surface of the elytra near the tips. He recorded the finding of *Dendroctonus simplex* breeding in the American larch in West Virginia at an elevation of 2,600 feet, and concludes that *D. simplex* may yet prove to be distinct from *D. rufipennis*. He further presented some interesting notes on the insect enemies of Scolytidæ.

This communication was discussed by Messrs. Schwarz, Howard, Ashmead and Johnson.

Mr. Ashmead stated that all of the European parasites of *Scolytus rugulosus* have now been found in the United States, the first one, *Chiropachys colon*, having been recognized 20 years ago by Mr. Howard.

Mr. Johnson stated that he had studied *Scolytus rugulosus* in the orchards of Maryland during the past few years. He found that it attacked plums and peaches with great virulence, but he had always noticed that the trees thus attacked had always been damaged in some way either by being barked in process of cultivation or by a branch being broken, or by some unknown cause. He had seen the *Chiropachys colon* in considerable numbers and had observed a curious habit in this insect in that both male and female when about to mate posture before each other vibrating the wings. One orchard of about 600 trees of the Satsuma plum had been extensively infested by *S. rugulosus*. The trees had died from some perfectly obscure cause which neither he nor Mr. Woods, of the Division of Vegetable Pathology, had been able to ascertain, and were immediately attacked in great numbers by the Scolytids.

Dr. Hopkins stated that this beetle will attack for food the buds of perfectly healthy trees, and in this way bring about so great an injury as to induce a breeding attack of the same insect.

This statement was confirmed by Mr. Schwarz, who said that in his opinion the insects of the genus Scolytus will attack perfectly healthy trees. He instanced the *Scolytus quadrispinosus* on perfectly healthy hickory trees at Detroit, Mich. These were old but perfectly healthy trees and they were not appreciably damaged by the insect. At Mt. Airy, Ga., he had seen an apparently perfectly healthy peach tree suddenly attacked by this insect for feeding purposes. The feeding punctures can always be distinguished from breeding punctures by the fact that they occur in circular rows.

Mr. Johnson stated that very few of us are able to ascertain what is a perfectly healthy tree, and that he was certain that in his experience some injury, however obscure, preceded attack by this insect.

Mr. Ashmead spoke of and illustrated by diagrams some important structural characters in the Crabronidæ. He had recently devoted

some weeks' study to the insects of this family and called attention to the excellent use which may be made of characters in the mandibles, palpi, antennæ, frontal fovea, clypeus, wings, abdomen, pygidium and legs. He would give generic rank to the sub-genera of Fox and Kohl, and would divide the family into four sub-families. He showed that all of the Fabrician species of *Crabro*, 17 in number, have been placed in other genera, and he finds himself embarrassed to indicate the type of the genus *Crabro*.

L. O. HOWARD,
Secretary.

BIOLOGICAL SOCIETY OF WASHINGTON—298TH MEETING, DECEMBER 30TH.

PROFESSOR A. D. HOPKINS exhibited some diagrams illustrating a system divised by him for showing in a graphic manner the evolutionary development of families, genera and species.

Mr. Charles L. Pollard discussed 'Floral Asymmetry in *Chamæcrista*,' explaining with the aid of diagrams the peculiar irregularity in the corolla and calyx whereby the banner petal, instead of occupying a normal uppermost position, has undergone a torsion of 90° to the left. This remarkable discovery was made originally by Professor E. L. Greene, who considered that it entitled the Chamæcristoid *Cassias* to rank as a distinct genus. Other generic characters were pointed out by Mr. Pollard.

Mr. Herbert J. Weber spoke of 'The Affinities of *Casuarina*,' discussing the external resemblance of *Casuarina* to *Equisetum* and the very close resemblance to *Ephedra* of the Gnetaceæ. The theory of their probable derivation from this group was accepted.

The subject of Chalazogamy in *Casuarina* was discussed somewhat in detail, the speaker taking exception to Nawaschin's theory that Chalazogamy is a primitive type of fecundation from which porogamy has been developed. The principal reasons advanced for thinking Nawaschin in error were:

1. Porogamy is the general type of fecundation in the Angiosperms, and is of special interest, as regards the Monocotyledons, which doubtless had a separate origin from *Casuarina* and other Dicotyledons, but were, nevertheless, derived from the Gymnosperms, where uniformly a slightly different type of porogamy from that occurring in the Angiosperms exists. As in the Monocotyledons, porogamy developed from a Gymnospermous type of fecundation, the speaker thought it more reasonable to think that the same type of fecundation had also appeared first in the ancestors of the Dicotyledons.

2. From the universal presence of the micropyle in all Gymnospermous and Angiospermous plants.

3. From the universal location of the egg cell in all Angiosperms in close proximity to the micropyle, instead of the chalaza.

The speaker took the ground that Chalazogamy may probably be looked upon as a degenerate form of fecundation rather than a primitive type.

Mr. O. F. Cook presented a paper entitled 'Four Categories of Species,' in which it was claimed that the general problems of taxonomy are four in number, and that from the standpoint of the work of investigation they may be looked upon as practically distinct. The term species has been employed in treating all four lines, being used (1) for arbitrary section of lines of individual succession, the 'species' of phylogeny and paleontology. (2) The insular or segregated species, the original and still the leading use of the term. (3) The incipient species, more properly called the subspecies. (4) The artificially selected or hybridized 'species. It was insisted that the fact of segregation is capable of establishment by sufficiently careful and extended observation; that it gives our most important clew to the present tendencies of evolution, and that the term 'species' should be restricted to naturally segregated groups of individuals.

F. A. LUCAS,
Secretary.

THE NEW YORK SECTION OF THE AMERICAN CHEMICAL SOCIETY.

THE local Section held its regular meeting at the College of the City of New York on the 9th inst., Dr. William McMurtrie presiding, and ninety-five members and visitors were present. An unusually long and interesting pro-

gram was announced, of which the following papers were read:

'Preliminary Note on proposed Patent Legislation in its Relation to American Chemists,' C. C. Parsons.

'Atomic Weights as a Cyclic Function,' Thomas Bayley, England.

'Recent Progress in Photo-Chemistry,' L. H. Friedburg.

'The Commercial Electrolysis of Salt in the United States,' H. Carmichael.

'Notes on the Electrolysis of Salt,' J. D. Pennock.

On motion the Chair was authorized to appoint a committee of three to consider what action should be taken on proposed patent legislation.

The Chairman reported that the Chemists' Club had been duly organized and the rooms leased, and it was expected that all necessary furnishing would be completed in time for the meeting of the general Society in the Holiday week.

The announcement was made that the membership of the Section has passed the 300 mark, which, in accordance with the provisions of the new constitution, allows the Section four representatives on the Council.

The Secretary was, therefore, directed by unanimous vote to cast a ballot electing the following gentlemen to represent the Society: William McMurtrie, A. A. Breneman, C. A. Doremus and A. H. Sabin; and in the event of any of these being elected Councillors-at-Large, Durand Woodman, J. B. F. Herreshoff, E. G. Love, E. E. Smith, Geo. C. Stone and C. B. Voorhees as alternates in the order named.

The Executive Committee decided to postpone the next regular meeting of the Section to Friday, January 13th, to avoid following the midwinter meeting too closely. The General Secretary reports a number of papers already promised for the midwinter meeting, and all arrangements progressing favorably.

DURAND WOODMAN,
Secretary.

NEW YORK ACADEMY OF SCIENCES—SECTION OF PSYCHOLOGY AND ANTHROPOLOGY.

THE Section of Psychology and Anthropology of the Academy is now in its third year. Six meetings have been arranged for the current season. The Section meets on the fourth Monday evening of the month at 12 West 31st Street.

At the first meeting, which was held October 24th, Professor Cattell presented a paper upon anthropological tests and instruments, outlining the advance which has been made in methods and apparatus in the psychological measurements of Columbia students.

Reports of summer field work in anthropology were made by Dr. M. H. Saville and Dr. Carl Lumholtz, speaking of work in Mexico, and by Dr. Farrand and Mr. Harlan I. Smith, whose work was on the Northwest coast, principally in Washington.

The program of the second meeting, November 28th, included a paper by E. G. Dexter, on 'The Influence of the Weather on the Mental Activities of Children,' and one by Geo. V. Dearborn, on 'Involuntary Reactions to Pleasant and Unpleasant Stimuli.' Anthropological papers were contributed by Stansbury Hagar, on 'The Water Burial,' and by A. Kroeber, on 'The Eskimos of Cumberland Sound.'

C. B. BLISS,
Secretary.

HARVARD UNIVERSITY: STUDENTS' GEOLOGICAL CLUB, NOVEMBER 22, 1898.

MR. H. F. KENDALL offered an explanation for the formation of an over-hanging, rock cliff on the eastern flank of Mt. Passaconaway, N. H. Mr. H. T. Burr traced the evolution of explanations for the 'Origin of Eskers,' and concluded that this form of ice records is of sub-glacial origin.

Geological Conference, November 29, 1898.—Dr. R. A. Daly presented results obtained in connection with an attempt to express, mathematically and graphically, the optical characters of the vertical zone of amphiboles and pyroxenes. A formula was deduced, which showed the variation in the extinction-angle (read against the cleavage-trace) characteristic of a plane revolved in the vertical zone from the position (010) to the position (100). This formula is a special case of Michel Lévy's general expression for the extinction in any zone.

By successive applications of the formula, curves were constructed for negative amphiboles on rectangular coordinates, in which the ordinate indicates the value of the extinction-angle on (010), and the abscissa the amount of rotation of the 'plan mobile,' out of the plane of symmetry toward the orthopinacoid. These curves were plotted for amphiboles in which the optical angle is 50°, 60°, 70°, 80°, and the extinction-angle on (010), in each case, 10°, 15° or 20°. To these were added the analogous curves for $2V=90°$. The last were unlike the former in that they showed no maximum value of extinction between (010) and (100). When the optical angle is small, the maximum extinction may be found to be in a plane far removed from (010), contrary to the statement of Zirkel that the maximum must always lie in the plane of symmetry.

Secondly, a method for determining the extinction-angle of amphiboles and pyroxenes (010) was proposed. The object of this new method is to avoid cutting oriented sections, as this operation is manifestly impossible with many rock-forming varieties.

'Two Remarkable Explosions in the New York Oil District' were described by Mr. L. LaForge. On March 1, 1898, three hundred quarts of nitro-glycerine exploded in a magazine, about one mile east of Wellsville, N. Y. Structures in that village suffered much damage; chimney-tops fell and windows were broken inward. One week later, six hundred quarts of nitro-glycerine exploded in the new magazine on the same spot. In this case no serious damage to buildings in the village resulted, although the report and shock of the explosion extended much farther. When the former explosion took place the ground was frozen, but before the latter occurred it had thawed out. It is to this fact that the people of Wellsville attribute the difference between the results of the two explosions.

J. M. BOUTWELL,
Recording Secretary.

THE ACADEMY OF SCIENCE OF ST. LOUIS.

AT the meeting of the Academy of Science of St. Louis on the evening of December 5, 1898, Mr. H. von Schrenk presented by title a paper 'On the Mode of Dissemination of *Usnea barbata*,' and Professor L. H. Pammel presented by title a paper on 'The Histology of the Caryopsis and Endosperm of some Grasses.'

Dr. Theo. Kodis presented the results of some experiments on overcooling animal and vegetable tissues, in which it was shown that, as water may, under favorable conditions, be cooled to some distance below zero, Centigrade, without freezing—the temperature immediately rising to the freezing point the moment that freezing begins, and remaining there until the water is entirely solidified, then beginning once more to drop—so, when animal and vegetable tissues are experimented on, they may be cooled to a temperature decidedly lower than the freezing point, under favorable conditions, before freezing begins, but that, when it begins, the temperature at once rises to the freezing point (which is always somewhat lower than that of pure water), remaining there until the process of freezing is complete, when it once more begins to fall. The speaker gave a short account of the current theories as to the mechanical constitution of protoplasm, and discussed the bearing on them of the phenomena when the solidification of overcooled tissues began.

WILLIAM TRELEASE,
Recording Secretary.

NEW BOOKS.

Aperçus de taxonomie générale. J. P. DURAND. Paris, Felix Alcan. 1899. Pp. 265. 5 fr.

Natalité et Democratie. ARSÉNE DUMONT. Paris, Schleicher Frerès. 1898. Pp. 230.

Catalogus Mammaleum tam viventium quam fossilium. E. L. TROUSSART. Berlin, R. Friedländer und Sohn. 1898. Fasciculus IV. and V. Pp. 665–1264. 26 Marks.

Principles of Biology. HERBERT SPENCER. New York, D. Appleton & Co. 1898. Revised and Enlarged Edition. Vol. I. Pp. x + 706. $2.00.

Degeneracy. EUGENE S. TALBOT. London, Walter Scott, Ltd.; New York, Charles Scribner's Sons. 1898. Pp. xvi + 37. $1.50

Psychologie der Veränderungsauffassung. L. WILLIAM STERN. Breslau, Preuss und Yünger. 1898. Pp. viii + 264.